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Section of Balneology and Climatology.

[*May* 10, 1930.]

Torquay as a Health Resort.

By K. R. Collis Hallowes, M.B.

In a paper dealing with the advantages of any definite place as a health resort, it is easy to exaggerate its claims, to view the perspective with local bias, to place too much importance on factors which it possesses in common with other places in the same locality, and to reiterate what is already well known. The avoidance of these difficulties is not very simple, as to exclude what has already been brought forward would leave many factors of importance unnoticed.

From the general and climatic aspects the subject has been considered on other occasions. Even in 1850, and earlier, articles had appeared on the subject. From the balneological and marine spa aspects, developments are more recent, although the original baths were opened in Torquay in 1817. They have been recently completely reorganized and modernized. In addition, a local natural mineral water is now available. It is generally recognized that marine health resorts are of considerable importance and that the English marine resorts are of considerable therapeutic value and present variety greater than that possessed by most other countries. How far position, altitude, barometric pressure, humidity and the chemical composition of marine air, amongst other factors, are of importance, is difficult to evaluate.

At most South Coast resorts, invalids and delicate persons—young and old—those whose resisting powers are below normal, are able to spend considerable periods of the day out of doors at all seasons of the year. They can wear lighter clothing than at more northerly and easterly places and are shielded from the sudden climatic variations which are more suitable for, and beneficial to, the robust. These advantages are real and important, and the recent work of Professor Hill and others demonstrates that conditions which permit of more exposure of the body to the atmosphere, even in the absence of sunshine, excite body metabolism and increase the resisting powers.

In the year 1780 Torquay consisted of only a few fishermen's and other cottages, and twenty years later its population was only 838. It gradually developed to 6,000 in 1841, but during the next ten years it more than doubled and reached 14,000 in 1854. This marked increase was explained in 1857 by Dr. Radclyffe Hall, a Torquay physician, "as due entirely to the preference of the public, and not to any especial favouritism on the part of the medical authorities at a distance," and in 1877 Dr. Spencer Thomson wrote: "The past of Torquay is chiefly remarkable as the history of its rapid rise, from an obscure fishing village, to a wide extended and beautiful town, a rise, moreover, entirely due to its advantages as regards climate, position and surroundings."

The population is now estimated at 42,000 with a marked increase during certain seasons of the year. There has been distinct development, and development continues, but Torquay is still free from factories which lead to smoke and pollute the atmosphere. It is a well-recognized health resort, a popular resort and one of repute, and it presents advantages more or less its own.

On account of its topography, it possesses several climates. Residence in certain parts means residence at or near sea level, southerly exposure and shelter from north and east. In other parts with varying exposures, residence is available from sea-level to an altitude of 400 feet. The physician and invalid have, therefore, a wide choice of situation and climate, and it is important that a wise choice should

be made in the first instance. A change can be made later, and it is often advisable to change from sea-level to a higher altitude at the end of the winter, and from a high altitude to a lower at the end of the autumn. This possibility of change of altitude and change of climate at the same resort is an advantage which should not be overlooked.

It is more or less easy to classify climates as soothing or stimulating and to state from meteorological, topographical and other data what the effect of a climate should be, but it is extremely difficult to state what the effect really is, and in Torquay this difficulty is increased by the differences which exist on account of the different altitudes and exposures.

The reputation of Torquay as a health resort is due to its mild and equable climate. The mean daily range of temperature is only 9·2° F. The mean temperature is 51·4° F. in summer; in winter, 44·5° F. These figures show extremely mild conditions for the British Isles. There is no sudden variation of temperature at sunset and there is an absence of the rapid variations of temperature which are experienced in so many places.

Sunshine averages about 1,800 hours yearly and the winter sunshine is about 29 per cent. of the possible, which is a high figure, and in excess of that recorded at most other British health resorts. The mean daily units of ultra-violet rays are high. The rainfall, which is often heavy, averages 33·9 in. a year, but what is important is that the number of rainy days is comparatively low and bears favourable comparison with resorts having a much lower rainfall. About 63 per cent. of the rainfall takes place between 6 p.m. and 8 a.m. The mean relative humidity is only 78 per cent. There is very little clay in any portion of the area, so that the rain rapidly disappears through the pervious rocks and soils.

From the balneological aspect it is recognized that a well-equipped baths establishment is a real asset to a health resort, and that a marine resort is a suitable place for physical treatment. Dr. Fortescue Fox has clearly expressed it thus: "The marine health resort is the place *par excellence* for physical treatment, that is to say, for the scientific use and combination of physical energies operating upon the surface of the body."

Torquay is fortunate in possessing such an up-to-date baths establishment, which is under the control of the Corporation. The Baths Committee of the Corporation work in conjunction with, and are guided by, the Baths Committee of the Torquay Division of the British Medical Association. This committee affords a valuable link between the profession and the local authorities. The equipment at the medical baths is modern, and most of the baths, douches and accessory treatments which have the support of medical hydrologists, are available and, with a few simple exceptions, treatments are only given on medical prescription. There is also a large tepid sea-water swimming bath and a vita-glass sun lounge. Sea-water or fresh water can be used for all baths and douches. Seaweed baths are considerably used. The bladder-wrack is gathered in Torbay, the fucus is extracted and about two gallons of the extracted material are added to an ordinary reclining bath of sea water or fresh water as prescribed. Dartmoor peat is extensively used for packs. The accessory treatments comprise electricity in its various forms, diathermy, radiant heat, ultra-violet radiation, Plombières douches, massage and exercises.

Sea-water has a more stimulating effect than fresh water, and diaphoresis after a sea-water bath is generally more pronounced than after a fresh water bath at the same temperature. It does not evaporate as quickly as fresh water, and salt particles are left on the skin. It is a good medium for massage by the Aix and Vichy methods.

Sea-water is a natural mineral water and the most abundant in the world, and the constancy of the relative proportion in which its constituents occur is a striking feature. It is safe to state, as Dr. Tyson did some years ago, that if it were more inaccessible and more expensive to use it would probably be held in higher esteem.

Seaweed baths are more hypertonic and more sedative than sea-water baths, and they leave the skin softer. Experience proves that balneological methods of treatment, when suitably prescribed, give good results in Torquay. An "after cure," as it is termed, is seldom necessary after a course of treatment. It appears advisable for elderly persons, invalids and children to reside at or near sea level while having baths during the winter months, or if higher up, only where shelter is adequate, but on account of the mild and equable climate balneological methods of treatment can be safely taken all through the winter. In fact, from the climatological and balneological aspects, Torquay has an all-the-year-round season. There are ample facilities for sea bathing, and many safe and pleasant beaches, on all of which coffee, bovril and other refreshments can be obtained.

Within the last few years Torquay has developed its own natural mineral water which had previously been neglected. It is bottled at the source and is available at the medical baths and elsewhere. The water is a natural cold water of the highest degree of organic and bacterial purity. It is hypotonic—of low mineralization. It has a slightly saline taste. Chemical and bacteriological examination after a period of six months in bottle showed no change. The analysis in parts per 100,000 is as follows:—

Electro-positive Ions.		Electro-negative Ions.	
Sodium ($Na^{\cdot}$)	4·8	Chloride ($Cl^{\prime}$)	12·3
Potassium ($K^{\cdot}$)	1·03	Nitrate ($NO_3^{\prime}$)	1·06
Ammonium ($NH_4^{\cdot}$) ...	Trace	Bicarbonate ($HCO_3^{\prime}$) ...	43·7
Magnesium ($Mg^{\cdot\cdot}$) ...	7·4	Silicate ($SiO_3^{\cdot}$)	1·64
Calcium ($Ca^{\cdot\cdot}$) ...	11·1	Sulphate ($So_4^{\prime\prime}$)	13·3
Iron ($Fe^{\cdot\cdot}$)	0·01		

Carbon dioxide (CO_2) 23·2 c.c. per litre at N.T.P.

The ions it contains are similar to those in the natural mineral water of some spas whose reputation dates back many years, but are present in larger quantity.

Research work in connection with natural mineral waters is difficult, laborious and costly, and frequently inconclusive in its results, and therefore conclusions are generally formed on clinical observations. In suitable cases in which the Torquay natural mineral water has been used, results have been satisfactory. It is generally prescribed for use three quarters of an hour before meals in the quantity of about half a pint. One pint in half-pint portions is frequently given before breakfast. Besides the flushing effect produced by the imbibition of a larger quantity of water than is usually taken, there appears to be a diuretic action, and the increased flow of urine carries away soluble waste products. In addition the increased fluid is likely to produce more efficient intestinal evacuation and the activity of the skin is increased. This increased activity of the skin is of considerable importance during a course of balneological treatment. Baths and douches and other physical treatments which act on the skin and stimulate it to increased activity, are very materially aided in their action when sufficient fluid is taken by the mouth, and experience teaches that when an increased quantity of water is prescribed, it is easier to have the instruction complied with if a definite natural water is specified. The psychological effect of drinking a natural mineral water at a health resort of repute must not be lost sight of.

Further, in many conditions, most benefit is obtained when reliance is not placed solely on baths and physical treatment, or on drinking of water alone, but on a combination of both. Climatic conditions which increase skin activity are also of importance in this combined treatment; a climate which permits of it all the year round is advantageous. The natural mineral water, sea water, normal saline and other solutions are used for intestinal irrigations by the Plombières method, and the indications for each are necessarily different, as the effect of the

injection of a hypotonic, isotonic, or hypertonic solution into the bowel must produce different effects on account of osmosis. It is not necessary to produce here a complete list of diseases and conditions which might by some be considered suitable for climatic treatment, or to attempt to state whether there are any definite contra-indications to residence in Torquay. Many health resorts and spas publish detailed lists but they are open to objection, and are frequently more suitable for publicity than as statements of medical value. Many are comparable to the elaborate diet lists so frequently handed to a patient, perchance chemically and scientifically accurate, but compiled with little thought as to what the individual concerned is able to digest, and quite indifferent to the personal idiosyncrasy which so frequently exists. It is the same when considering change of climate. It has been accurately stated that each place has its own advantages, suited to the varied cases and constitutions, and it is in this respect that the physician must discriminate without fear or favour. In Torquay it is also necessary to remember, as has been previously pointed out, there is a distinction and a varying one between "Torquay within the hills," and "Torquay upon the hills." The deciding factor is the individual's response to his environment.

The mild and equable climate experienced here is generally beneficial for bronchial affections and catarrhs, and for those conditions which benefit from increased circulatory and glandular activity of the skin.

Elderly persons, children and convalescents all derive great benefit, and they form probably the most important group. Persons who have returned from the tropics, and those who, on account of the strain engendered by residence in more northerly climates, require relaxing, should in general receive benefit here. Particular mention is necessary of the advantage which is likely to be gained by those suffering from the strain and stress of modern business life, the breakdowns of middle life, and insomnia. In most of the older writings about our climate, particular emphasis is laid upon its advantages in pulmonary tuberculosis. There seems in general to be considerable divergence of opinion in the profession as to whether "rheumatism," using the term in its widest sense, is beneficially or detrimentally affected by residence at marine resorts. This divergence could be lessened or eliminated, if every ache and pain did not necessitate a diagnostic label, if osteo-arthritis, infective arthritis and some other conditions lost their rheumatic designation. There appears to be no definite evidence to prove that climate has any ætiological significance in these conditions, but experience teaches that a mild climate, absence of extremes and abundant sunshine benefit this group, whereas the combination of cold and damp is frequently detrimental. In general the majority of sufferers from the so-called rheumatic conditions do well in Torquay both from the climatic and balneological aspects. Torquay as a health resort is likely to develop still further and, as in all other health resorts and spas, the development should be that of its own natural resources.

Section of Anæsthetics.

[*May* 2, 1930.]

Ethylene Anæsthesia.

By R. J. Clausen, M.C., M.B.

The first mention of ethylene anæsthesia in the literature is as early as 1849, when Thomas Nunneley, a surgeon of Leeds, published the results of his extensive investigations into the anæsthetic properties of 34 various substances.

Ethylene was not considered likely to be of use, on account of the difficulties of preparation, etc., and of its uncertainty and narrow margin.

His results with nitrous oxide were also disappointing, and convinced him that it was by no means harmless and could never be used as an anæsthetic.

Ethylene anæsthesia dates from 1923 when Luckhardt, of Chicago, and others first demonstrated its use and reported cases.

Its vogue spread rapidly in the United States and in Canada, but in this country it has never been widely adopted.

Fairlie, of Glasgow, however, has reported a considerable experience in the Journals, while Sir Francis Shipway and Dr. Langton Hewer both published quite favourable reports in 1925.

The formula for ethylene is C_2H_4. It is a colourless gas with a characteristic smell, part of which may be due to impurities; it is highly inflammable, and when mixed in suitable proportions with air, oxygen or nitrous oxide, it is explosive.

When used in a M'Kesson or Walton machine calibrated for nitrous oxide the oxygen percentage actually given will not necessarily be that shown on the dial. Theoretically it should be less, but by the courtesy of the Director of the Research Laboratory of the British Oxygen Company I had a number of analyses made of the mixtures delivered, with curious results which showed I had been using much more oxygen than I had thought.

Ethylene is manufactured and supplied by the B.O.C., the cylinders corresponding in size to those for nitrous oxide, but containing less, since the gas is not liquefied; the 150-gallon ethylene is equal to the 200-gallon gas. Since it is an inflammable gas, the valve nozzles have a left-hand thread, therefore if a Boyle's machine were used, special adapters would be required; personally I much prefer the yoke attachment with straight or "type 7" valves.

I have now given 371 administrations, of which 92 were for dental extractions, the remainder for surgical operations; actually, from force of circumstances, these were largely gynæcological. In all, except a few of the dental cases, the operations were of sufficient length to demand the use of oxygen, and in all the anæsthetic has been given with the M'Kesson machine, except again in some of the dental cases, in which a machine made by the Amalgamated Dental Company was employed. Any gas apparatus can, of course, be used. The ages have ranged from zero to 84, 0 being represented by 5 Cæsarean sections. The duration of anæsthesia has extended in one case up to 2 hours 25 minutes; in 4 it was 2 hours or more; in 37 from 1 to 2 hours; in 92 from ½ to 1 hour, and in 146 half an hour or less, excluding

the dental cases. In 14 a spinal anæsthetic has been given as well, 3 being "spinal" failures, the remainders being given as a combined anæsthetic. This method gives excellent results for pelvic operations, and might serve for upper abdominal operations also. A few administrations have been endotracheal, usually with a single wide tube.

Morphia and atropine together have been given in about three-fourths of the cases, atropine only in about a fourth. The preliminary use of some hypnotic is, of course, desirable.

The actual technique of administration is that used with nitrous oxide. Nitrous oxide may be used for induction, but this should not, as a rule, be necessary, since I can say from personal experience, as well as from reliable hearsay, that the induction with ethylene is quick and pleasant. I have, as a rule, begun with pure—or nearly pure—gas, increasing the oxygen percentage slowly after the first few breaths. On a few occasions I have unduly pushed the gas during induction and stopped the breathing, but this condition has soon yielded to pure oxygen, sometimes under pressure, since as a rule the respiration fails well before the circulation. This production of "secondary saturation" has never been intentional. The time of induction is short, probably less than that with nitrous oxide; the quick and sudden onset of anæsthesia with relaxation has often been commented on by surgeons. The type of anæsthesia resembles that produced by nitrous oxide, but is deeper and smoother; there is more relaxation, while pulse and respiration are as a rule slower. The colour is better, since the oxygen percentage employed is considerably higher, but should an oxygen shortage occur, the signs of stertor, cyanosis, etc., may not be so obvious as with nitrous oxide.

The maintenance rate for oxygen naturally varies fairly widely in different cases and, to some extent, at different stages of the administration. The usual range has been from 10 to 15% as shown on the dial, which, on my machine, apparently represents from 15 to 30%, the usual amount used being about 20%. Consumption of gas has on many occasions averaged about 60 gallons per hour, entailing an oxygen consumption of about 2 feet per hour, which would bring the cost for hospital use to about 3*s*. 3*d*. per hour. One 150-gallon cylinder used on four private cases lasted for 3¼ hours, giving a consumption of about 46 gallons per hour, while another 150-gallon cylinder gave over 200 minutes anæsthesia, or less than 45 gallons per hour.

Ether has been used in nearly all abdominal operations to give adequate relaxation, unless a previous spinal anæsthetic had been given, but the amounts used have been small; in some cases it has been used only for the peritoneal closure, in others at the beginning only. Probably with care, and by giving a heavier pre-medication and longer time for induction, also with some coöperation from the surgeon, it could be more often dispensed with. Small amounts of ether are, as a rule, notably more effective with ethylene than with nitrous oxide. As regards the amount of ether used, three ounces is a large quantity for an abdominal operation, and has only been exceeded on about twelve occasions. In twenty-six abdominal operations, lasting one hour and over, without spinal or local anæsthetics, the amount has averaged just over two and a half ounces, the duration of anæsthesia averaging eighty-two minutes. Chloroform has never been used in combination.

For such operations as the many minor gynæcological ones, amputations of breast or extremities, ordinary hernias, etc., ether should not be required, while for abdominal operations in debilitated or shocked persons, the amount needed is very small. It is in the latter class of case that ethylene has been notably useful, and in the repeated anæsthesias for radium treatment of advanced uterine carcinoma. It is also of value in dental work in hospital, where large numbers of extractions are done in a short time on refractory patients.

I do not of course claim that all my cases have benefited from ethylene, and I

admit that many would have done as well or better with other anæsthetics; on the other hand, in no case has it been necessary to abandon ethylene.

Recovery is quick, nearly as quick in the first place as after nitrous-oxide anæsthesia, though a longer time is taken for complete elimination of the gas. The condition, after prolonged anæsthesia, is often strikingly good, and convalescence is as good as it is after the use of nitrous oxide. Reports in the literature show that ethylene causes less disturbance of function or damage to tissue than any other general anæsthetic. Vomiting once or twice on recovery is common in cases which have lasted over about a quarter of an hour, but serious or troublesome after-sickness is rare, though more frequent than after nitrous oxide. As regards ill-effects or sequelæ, though there have been a certain number of deaths, none, I believe, could be in any way attributed to the anæsthetic. One Cæsarean baby died six hours after delivery, with unexpanded lungs, though it had cried at once when born. Another baby was very white and collapsed when delivered, and afterwards jaundiced for several days. Three other Cæsarean babies did very well. Ethylene being a novelty in the ward was of course blamed, but probably unjustly.

The drawbacks to the use of ethylene are commonly stated to be: (1) Its smell, (2) increased hæmorrhage, (3) risk of fire.

(1) The first need not be considered seriously, as with an efficient exhaust pipe it is, for all practical purposes, eliminated. By this I mean with a pipe leading the expired gases away from the patient, preferably outside the theatre. The one I am using is a rubber hose of a half-inch bore, and has the advantage of going inside the case of the machine and so being available in nursing homes and private houses. If the use of ethylene were seriously considered in a hospital it would be very easy to lay a pipe of wide bore in the floor, to which a flexible tube from the mask could be connected. I have no doubt that objections may be raised to this method, but I have used it in all but the first six cases and have not noted any ill-effects. Under normal conditions of anæsthesia it only causes an increase of expiratory pressure in the mask of about $\frac{1}{2}$ mm. of mercury or less. The same results could also be obtained with a closed circuit and carbon-dioxide absorption, which I have not tried.

(2) The liability to bleeding often seems increased, even in the absence of any congestion or cyanosis. However, many cases bleed very freely when under ether or even chloroform, and I do not think that ethylene can be condemned on this ground.

(3) The danger of fire or explosion constitutes a real risk, which, however, should be considered reasonably and not exaggerated. It certainly forbids the use of ethylene in the presence of naked lights, cauteries, diathermy and so on. Most of the fires and explosions which have occurred in the United States appear to have been due to neglect of such obvious precautions, or to static electricity, a phenomenon which is not met with here to anything like the same extent. In one case recently reported, however, the explosion apparently occurred in the mask or breathing-tube of the machine, with fatal results to the patient. At the time a very weak mixture was in use, which probably increased the danger, for there appears to be little risk of explosion with 20% or less oxygen. The only special precaution I have taken is to put a little water into the re-breathing bag to moisten the mixed gases and lessen the remote possibility of static sparks in the bag. With reasonable skill and care there should be no chance of ignition from outside.

To sum up, I believe that ethylene has many advantages over nitrous oxide, and can often replace the latter with benefit.

On certain occasions, however, it must be definitely forbidden, and its drawbacks will probably prevent its general adoption as a routine anæsthetic, though it deserves a more extended and thorough trial.

Mr. C. LANGTON HEWER said that after an experience of about 140 cases he thought that this gas offered two advantages over nitrous oxide, namely, greater muscular relaxation and

the feasibility of using a higher percentage of oxygen. Against this must be placed, as Mr. Clausen had said, its offensive smell, its explosiveness when mixed in certain proportions of air or oxygen, and the greater tendency towards nausea and vomiting after administration. In his (Mr. Hewer's) opinion, nitrous oxide and oxygen with minimal ether gave the advantages of ethylene with rather less than its disadvantages, so that he had now discontinued the use of the latter gas.

The smell of ethylene was a curious feature, since when the gas was freshly made from sulphuric acid and ethyl alcohol, it had only a faint ethereal odour, but when it had been stored the offensive acetylene-like smell developed. He (Mr. Hewer) understood that research was now in progress with a view to avoiding the decomposition of ethylene, and if this was successful he thought that the gas would come into greater favour with anæsthetists. The odour was much more apparent to the bystanders than to the patient and could be abolished entirely by using a closed-circuit machine. The disadvantage of this, however, lay in the bulkiness of the carbon-dioxide filter, and the difficulty of telling when the soda lime should be changed. As a compromise the exhaust pipe described by Mr. Clausen was useful.

When speaking of the history of ethylene, the name of the late Dr. Mackenzie Wallis should not be forgotten. It was his work in 1920 on the enhanced anæsthetic effect obtained by dissolving ethylene in pure ether that stimulated others to investigate the action of the pure gas.

Section for the Study of Disease in Children.

[*May* 30, 1930.]

PRESIDENT'S ADDRESS.

Some Remarks on Osteogenesis Imperfecta.

By H. A. T. Fairbank, D.S.O., M.S.

The subject of this address has been burdened with a multitude of names. Some we now recognize as misleading; others are difficult to spell and more difficult to pronounce. I prefer "osteogenesis imperfecta" because it is descriptive and, in this country at any rate, is the name by which the disease is most commonly called. The name should, I think, be applied to the affection met with in infants and children, the chief feature of which is the fragility of the bones.

It is usual to divide the cases of osteogenesis imperfecta into at least two groups namely, those in which fractures occur before birth, and those in which the infants are apparently healthy when born and the fragility of the bones is only revealed at a later date. I say "apparently" advisedly, since there are many reasons for believing that in both groups the affection begins during intra-uterine life. It is, I believe, always congenital; the difference is rather one of severity.

John Thomson [1] stressed the importance of the familial influence and the occurrence of blue sclerotics in the group with a post-natal onset. Let us inquire into these two points. No one can deny that bone fragility and sclerotics with a deep blue tinge may occur, either together or separately, in several members of a family; many such families have been carefully observed and recorded. Griffiths [2] says that the hereditary influence is to be found in 27%. In the small series of 28 cases of which I have been able to collect radiographic and other details, largely owing to the kindness of my colleagues, a familial or hereditary influence was evident in 28%. Of 13 cases in which the first fracture was known to have occurred at or before birth, in only one was there any evidence of the affection in another member of the family, while in 14 post-natal cases 7 gave a history of fragility in the family. In five of these post-natal cases the first fracture, be it noted, occurred in infancy. Two were twins. In the only ante-natal case, the patient, a girl, had one brother affected (a post-natal case) out of a total family of four. The brother had sustained his first fracture at the age of 6 months. Neither of these children

had blue sclerotics. In many of the post-natal cases examined, the first fracture had occurred soon after birth, and these might well be regarded as ante-natal cases of mild severity. As to blue sclerotics, these were noted in 11 cases, four of which were ante-natal; half the post-natal cases showed this feature. Thomson [3], by the way, says he has never seen blue sclerotics in an ante-natal case. The blueness of the sclerotics in any given case may well be the subject of dispute, as so many infants have blue sclerotics. It is necessary to limit the term to cases in which the blueness is definitely more marked than is seen in many healthy infants. Moreover there appears to be a tendency for the colour to fade. In one case (apparently ante-natal, since one tibia was bent at birth) in which the patient, when seen at the age of $2\frac{1}{2}$ years, had "rather blue sclerotics" the colour was said to have been much more intense soon after birth. When the child was seen at the age of 4 years on account of a recent fracture, the sclerotics were even less blue, and certainly well within normal limits.

It would seem, therefore, that although familial and hereditary influences are more common in the post-natal than in the ante-natal cases, and that blue sclerotics are found with greater frequency in the former, there is no clear distinction between the two. I have not been able to find any relation between the colour of the sclerotics and the number—more or less—of the fractures, that is, between blue sclerotics and the severity of the disease.

It is of interest to note that the typical broad head, described by Cameron [4] was present in at least ten of the cases, five being ante-natal, and five post-natal. Thomson [5] says many, if not all, develop this sign. It seems to me to be associated with a grave severity of the disease. Dwarfing is a marked feature in many—in fact, I think in all but the milder—cases.

In a previous paper [6] I ventured to separate the cases into four groups showing different and characteristic radiographic appearances. These may be briefly designated as: (I) The thick bone type. (II) The slender, fragile bone type. (III) The honeycomb bone type. (IV) The marble-bone type (Albers Schonberg's disease).

Type I.—The features of the first group are best seen in a severe case soon after birth. The limbs are stunted, the whole appearance of the child being not unlike that seen in achondroplasia; the major long bones, as seen in a skiagram, are short and unusually broad and thickened, and show one or more fractures with an abundance of callus. The proximal segments of the limb, the femora and humeri, are most often affected and usually symmetrically, while the tibiæ may also be enlarged.

Type II.—Includes the larger number of cases met with. The bones are usually, but not always, slender, with a thin cortex and a general want of density. There is an appearance of intense atrophy, while the extremities of the shafts may show some signs of honeycombing. Bowing is common, and this may seem to be the result only of the fractures, or of bending of the shafts in addition.

Type III.—This has only been met with once to the degree seen in these slides [slides exhibited], though, as already stated, a certain amount of honeycombing of the metaphyses is not uncommon. The patient in this case was $5\frac{1}{2}$ years old when first seen four years ago; she is apparently in quite good health to-day, though still unable to walk. The affection is said to date from birth but no detailed history is obtainable of the first and subsequent fractures. The sclerotics are not blue. The honeycombing appears to be distributed throughout the whole of the skeleton, but not in a uniform manner. In the leg bones the honeycombing is pretty general, while in the arms the metaphyses alone are cystic. The deformities seem to be the result partly of fractures, and partly of bending of the softened bones. The pelvis is noticeably deformed. As this child was remarkably well I set out to correct the chief deformities in her legs and began with one tibia. I think the mistake was made of not removing enough bone to relieve all tension; in spite of all efforts to prevent it, the callus yielded and the deformity returned.

A section of a fragment of bone removed at this operation shows the numerous large spaces to be filled with fibrous tissue (fig. 1). I have never met with another case showing such a marked degree of honeycombing. My impression is—it may be wrong, of course—that Von Recklinghausen's disease never gives an appearance quite like this. We can confine our attention, therefore, to the first two groups. Type I is only seen in ante-natal cases, and then only in the first months of life. Type II is by far the commonest type in my experience. It was seen in twenty of the twenty-eight cases of my series; in a quarter of these the condition was definitely ante-natal in origin. It seems highly probable that if a patient lives long enough, the radiographic appearance characteristic of Type I will gradually change

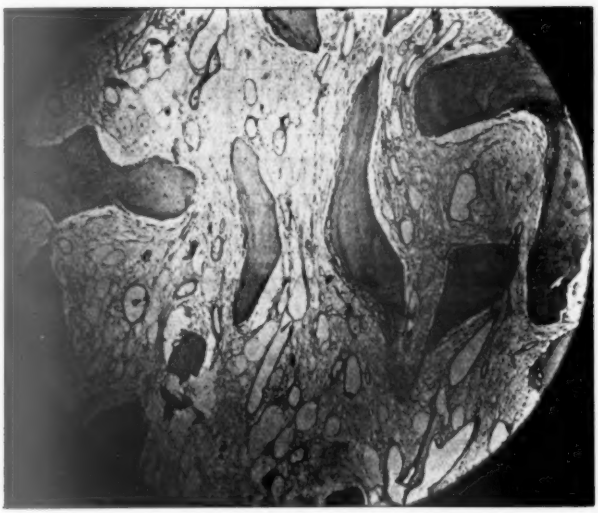

FIG. 1.—Section of fragment removed from case showing honeycombed type. Spaces in bone filled by loose vascular fibrous tissue.

into those characteristic of Type II. The oldest child met with showing the radiographic appearances of Type I was eight months old, while the youngest seen with Type II bones was nine months old.

In this latter case the affection started before birth; in spite of many fractures of ribs and other bones, life had been sustained for nine months. It is rather difficult to explain the stoutness of the femora and humeri in the congenital cases (Type I), since the enlargement is remarkably uniform, except for the callus about a recent fracture. The structure of the bones, except in the proximity of the fractures, is of uniform density and does not in the least suggest that the increased thickness is due to callus formation. Moreover, the enlargement extends right up to the epiphyseal lines, as is well seen in this photograph of a specimen now in the

museum of the Hospital for Sick Children (fig. 2). It is remarkable that this increase in diameter is only apparent in the bones mentioned, viz., the femora, the humeri, and, less commonly, the tibiæ, and not in the other long bones. The affected bones, it must be admitted, are the seat of obvious fractures more commonly than the other long bones, with the possible exception of the ribs, which often show a large number of fractures with a redundant amount of callus. On the whole, I am not quite convinced that the occurrence of fractures, with the resulting excess of callus, in these bones can explain their stunted, stubby appearance, though it may be so. It is well known that, in some cases, bending of the bones occurs without antecedent fractures, which alone could not explain all the deformities found in the patients; this is not, however, very common. In only five of the twenty-eight cases was this sign of an osteomalacic condition of the long bones definitely present, four of these being ante-natal in onset. In seven cases the pelvis was deformed, i.e., indented; four of these corresponded to four of the five cases showing bending of the long bones of the limbs.

Scoliosis, which is regarded by some as a sign of want of solidity of the vertebræ, was present in eight cases; naturally this deformity was found in the severe cases

FIG. 2.—Photograph of specimen from child 2 days old, in museum of Hospital for Sick Children, Great Ormond Street. Note enlarged stunted shaft, with evidence of fracture only about its centre.

which had managed to live for several years. A point which sometimes arises is whether severe rickets can cause a degree of fragility comparable with that seen in undoubted cases of osteogenesis imperfecta. A case shown by Dr. Langmead at a meeting of this Section in 1920 [7] as one of "osteogenesis imperfecta plus rickets," was a good example, and gave rise at the time to differences of opinion. The diagnosis of rickets was unquestionable, but the fractures were transverse—not greenstick—and were numerous. The same difficulty arose in two cases reported by Gorter [8], to which reference will be made later.

The Teeth.—An interesting feature of these cases is the imperfect calcification and abnormal translucency of the teeth, since the enamel, which is unquestionably deficient in lime salts, is epidermal, i.e., epithelial in origin, while the bones are, of course, mesoblastic. I have noted occasionally that the permanent teeth are better calcified than the temporary, and this in spite of the fact that the persistent recurrence of fractures suggests there has been no improvement in the fragility of the bones. This has been noted even in post-natal cases, in spite of the fact that much of the calcification of the crowns of the temporary teeth takes place before birth. At birth the extent to which the calcification should have proceeded in the temporary

teeth varies with the particular tooth examined and the date at which it is due to erupt, but roughly speaking, we may say that anything from the mere tip up to two-thirds of the crown should be calcified when the child is born. The crowns of the permanent teeth, on the other hand, are formed for the most part between the eighth and the eighteenth month after birth. Only one permanent tooth, the first molar, shows any calcification at birth. So far as the teeth are concerned, therefore, there is some evidence of improved calcification after birth, as compared with fœtal life, in spite of the absence of any marked improvement in the condition of the skeleton as a whole.

Ætiology.—We naturally turn to the pathology for help in this matter. There seems to be no doubt that the periosteum is generally thickened and fails to lay down cortical bone in a normal manner. As is seen in this slide of the femur of a child that died within twenty-four hours of birth, there is nothing in the way of a continuous layer of cortical bone; only isolated flakes of bone of various sizes are seen under the periosteum. The number of osteoblasts lining the trabeculæ is also said to be deficient. This has been noted by many observers, and is regarded by Knaggs [9] as being an essential factor in the causation of the disease. This seems to be true, to a varying extent, when the juxta-epiphyseal region of cases dying in early infancy is examined. With my limited experience of the normal and pathological histology of bone, I hardly venture to question Knaggs' conclusions, yet it is remarkable how freely the fractures in these cases unite, with certain exceptions to be noted later. In most, at any rate, callus is freely formed. Is not this callus laid down by osteoblasts? This section of a fragment removed close to a very imperfectly united fracture shows, I think, an ample number of osteoblasts. The case was a typical one in a child, aged 7 years, with a history of twenty-three fractures in three bones. Perhaps the stimulus of a fracture is necessary to bring these cells into existence in any numbers. Knaggs lays stress on his having found cartilage cells instead of osteoblasts formed by the periosteum, but I rather doubt this being more than an occasional change seen here and there. Anyway, is it very strange for these cartilage cells—large cells be it noted—to be found under the periosteum, which is itself derived from the perichondrium, and are not cartilage cells, presumably derived from periosteal cells, to be found in callus at all ages? Even if we accept the relative failure in the formation of the osteoblasts, we have to admit, with Knaggs, that the cause of this failure is as yet unknown.

Let us turn for a moment to an entirely different side of the question, namely, the biochemical. In a previous address [10] attention was called to the researches of Robison, whose paper was published in 1923 [11] and whose work seems to me of the greatest interest and importance. Since that date further researches have been carried out by Robison and others. Briefly, Robison discovered an enzyme—phosphatase—which he regarded as an active agent in the process of normal ossification. This ferment effects the hydrolysis of certain phosphoric esters in the blood, and this results in the deposition of calcium phosphate. This phosphatase is secreted in the region of the osteoblasts and the hypertrophic cartilage cells, but none is found in the small celled cartilage of the epiphysis (Robison and Soames [12]). In other words it is plentiful only in the ossifying portions of normal bones of embryos and infants, and is absent from the non-ossifying cartilage. Perhaps I may remind you that it is found in appreciable quantities in only two other places in the body, namely, in the intestines and the kidneys. Fell and Robison 13] reviewed the subject last year and published a number of experiments carried out by cultivation *in vitro* of bone and cartilage removed from fowl embryos. They found in these cultures that the isolated chick femora synthetized a phosphatase, this enzyme having been absent at the time of explantation, and that the amount increased with the age of the culture. Moreover, the increase corresponded with the progress of the histological differentiation of the culture, but the maximum

ratio between these two was even higher than in normal embryonic femora. It is remarkable that these results were only obtained with five-and-a-half- or six-day embryos. If three-day embryos were taken, the culture failed to produce any phosphatase, since in these, hypertrophic cartilage cells failed to develop. Kay [14] states that the phosphatase is secreted by the osteoblasts and also by the hypertrophic cartilage cells. In osteogenesis imperfecta the osteoblasts may be deficient, and therefore, presumably, the formation of the enzyme necessary for the deposition of lime salts, but, on the other hand, the hypertrophic cartilage cells are not, I think, in any way deficient. In one of Knaggs' [15] slides he shows these cells formed even by the periosteum in abnormal numbers.

Robison and Soames [16] give six possible errors which might adversely affect the deposition of calcium phosphate. Three of these are biochemical and beyond my understanding, so I will not refer to them. The other three are: (1) failure of the cartilage or bone cells to secrete the necessary enzyme; (2) deficiency of the phosphoric ester in the blood: (3) alteration of the corpuscular membrane, capillary wall or the cartilage.

The ester, it may be mentioned, is chiefly found in the corpuscles. Unfortunately no researches, so far as I have been able to discover, have been carried out on the presence or absence of Robison's ferment in cases of the affection we are now considering. Estimation of the blood-calcium has, so far, given us no help. It may be either high or low. It is important to remember that the skeleton is not alone affected in these cases. The sclerotic coat of the eye is certainly abnormal in a proportion of the cases, while the teeth are imperfectly calcified in most, if not in all. Now the enamel of the teeth is deposited by epithelium, derived from the epiblast, and until it has been proved that this epithelium normally secretes the enzyme found in growing bone and that this enzyme is deficient both in the bones and in the enamel organs in cases of osteogenesis imperfecta, we are forced to look elsewhere for a cause which will explain all the various errors of development which we find associated in this disease. At present we know nothing of the stimuli which prompt the formation of an adequate number of osteoblasts, and make these cells secrete an enzyme as a preliminary to the deposition of bone. Though I have been able to throw no light on the difficult problems involved in the causation of this disease, these few remarks may, I hope, call attention to the urgent need of further research, particularly along the lines suggested by this discovery of Robison. One great difficulty is the scarcity of material for laboratory work; Dr. Robison has confessed to me a slight hesitation about embarking on researches which the dearth of material may make it impossible for him to repeat. This difficulty should not be insurmountable if Members of this Section and perhaps our gynæcological colleagues are willing to assist.

Treatment.—It is generally agreed that at present we have nothing to give in these cases with any hope of effecting a cure. In this connection I want to refer to two cases published by Gorter [17], of Leyden, to which Dr. Donald Paterson has kindly called my attention. In these two cases Gorter found that rapid amelioration of the fragility followed treatment with thymus. Both patients were rickety children and were vigorously treated for rickets without effect on the fragility, which, however, responded at once to the administration of thymus. In one case fresh pig thymus was given in 20-gramme doses daily, while in the other compressed thymus gland was used. Unfortunately no skiagrams, before and after treatment, have been published. The difficulty about the cases is the question as to whether rickets could explain the whole clinical picture, and whether a cure was effected by the administration of thymus *via* the rickets. One is impressed by the fact that a physician with the high reputation enjoyed by Professor Gorter answers both these questions in the negative. He discusses them most thoroughly, referring to much experimental work, and comes to the conclusion that rickets alone was not

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PLATE I.

Skiagram of femora of a patient 7 years of age, showing imperfectly united fractures with gross deformity.

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PLATE II.

Skiagrams of left femur from case shown on Plate I, taken three months after operation to overlap fragments and straighten bone.

the cause, but that osteogenesis imperfecta was present in addition and was cured by the thymus. No doubt before long we shall have more evidence on the value of this gland.

Massage is recommended for the limbs, but in many cases it has to be carried out with the greatest care; at the best it can be of little use. Faradic treatment of the muscles should be more helpful, if only the children could stand it. It appeals to me as rational, for it seems hopeless to expect much in the way of solidification of the bones without some preliminary improvement in the muscles. For similar reasons it is highly desirable to get the patients on their feet, if only this is possible with the help of apparatus. Unfortunately in many cases it is quite out of the question. It is usually stated that callus is formed in a normal manner, and that the fractures unite readily enough. Though true of many cases, this is not true of all. It is not uncommon to find that the fractures occurring in a particular case are confined to two or three bones, which give way at the same spots, time after time. Sooner or later gross deformities develop, the union after each fracture having been very far from perfect. Such callus as is formed remains tender, showing that it is still soft, for weeks and even months, after the occurrence of the last definite fracture. It has been suggested that in some cases calcification only occurs, and that the callus is never really ossified. This is a possible explanation both of the recurrence of the fractures and of the bending of the callus. In many such cases, with their poorly nourished and wasted limbs, it is found impossible to maintain adequate fixation and extension for the prolonged period necessary before firm union occurs. In my experience it is rarely possible to pull out a limb, shortened by a mal-united bone, in such a case, and to maintain the length for the many weeks necessary. It is, therefore, of paramount importance to preserve, if possible, accurate alignment when treating the first—and any subsequent—fracture in a bone, for if any bowing is allowed to occur, the chance of refracture is greatly increased. This is of less importance, of course, in the arm than in the leg.

Lastly, I would like to suggest that the only way to deal with a case with marked deformity is to abandon all efforts at lengthening the limb. If, after due consideration, it is thought advisable to operate on such a case, the bone should be straightened by overlapping the fragments, so that perfect alignment and fixation can be maintained with ease. If there is the least tension on the muscles, bowing will probably recur. These skiagrams, taken before and after operation, are from a case operated upon when the patient was aged 7 years (Plates I and II). The ends of the fragments were freshened, bevelled, overlapped, and sutured together with catgut. Solid union with a straight bone seems to have occurred. My operative experience, I must admit, is limited. I have tackled only five cases, and two of these were simple osteotomies for genu valgum, but I cannot help feeling that the above is the right way to deal with the occasional case in which operation seems to be justified.

Summary.—In the time at my disposal it has only been possible to touch on a point or two here and there. I have endeavoured to give some of the reasons for regarding the ante- and post-natal cases as due to the same disease. It has been suggested that there is need for further investigation into the histological changes accompanying this disease, and that the teeth should receive attention as well as the bones. A single fatal case should furnish material for a complete examination of the dental tissues in all stages of development. I have referred to some of the research work which has been carried out into the secretion and action of Robison's ferment, and have stressed the great importance of this work in the study of osteogenesis imperfecta. We are anxious to know whether this ferment is to be found in normal amounts in growing bone in this disease, whether it plays a part in the calcification of normal enamel, and, if so, whether it is deficient or absent during the formation of the transparent teeth we find in these patients with brittle bones. With regard

to treatment two points have been specially referred to. First, the claim of Gorter to have cured two cases with thymus, and second, if operative correction of deformity in any given case is considered advisable, it is suggested that length of the deformed bone must be sacrificed to obtain perfect alignment, free from all longitudinal tension, and with as wide an overlap of the fragments as possible.

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[The report of the cases shown at this meeting will be published in the next issue of the *Proceedings.*]

Clinical Section.

[*May* 9, 1930.]

Tracheotomy for Diphtheria on Mother (1896), and Child (1930).—J. D. ROLLESTON, M.D.

The mother, aged 37, gives a history of having been tracheotomized on account of laryngeal diphtheria, at Perth, in 1896, when she was $3\frac{1}{2}$ years old. Four years later she was operated on for tuberculous glands of the neck, and the same year had another attack of diphtheria followed by loss of power in the legs.

Since 1900 she has enjoyed good health, and has not been subject to respiratory diseases. Her tracheotomy scar is $1\frac{1}{2}$ in. long by $\frac{3}{4}$ in. wide, and she shows scars on the left side of her neck at the site of the tuberculous glands. She has borne four other healthy children besides the following patient.

This child, aged 2 years, was admitted to hospital, on account of faucial and laryngeal diphtheria, on March 12, 1930. Tracheotomy had been performed the day before admission. The tube was removed five days after the operation. Recovery was uneventful, and on discharge from hospital on May 5, the tracheotomy scar measured $\frac{1}{2}$ in. long by $\frac{3}{8}$ in. wide.

Are we to regard these cases as mere coincidences, or as due to a special susceptibility of the larynx to diphtheritic infection? Probably they are mere coincidences. Most of us have seen tracheotomy scars in adults, but it is very unusual to see both parent and child who have had this operation. Last year I had hoped to show an almost identical case, but the patient failed me at the last moment.[1] In 1899, at a Tuberculosis Congress in Berlin, Professor Louis Landouzy [2] maintained that children who had undergone tracheotomy for laryngeal diphtheria were specially susceptible to tuberculosis, and that very few of those cases reached adult life, basing his statement on the fact that he rarely saw tracheotomy scars in recruits. He suggested that the tracheotomy destroyed the normal phagocytic defence of the trachea. A storm of protest was raised in many European countries against that generalization. The critics admitted that tracheotomy predisposed to chronic tonsillitis and respiratory affections, but in neither of the two mothers whom I have seen, who have undergone tracheotomy, has there been a history of subsequent respiratory troubles or chronic tonsillitis.

Discussion.—Dr. W. M. FELDMAN said he was convinced that the statement that children tracheotomized after diphtheria did not survive to adult life was not true, but it was remarkable how few adults could be seen with scars in the neck resulting from operation in childhood.

Dr. ROLLESTON (in reply) said that in pre-antitoxin days the proportion of survivals after tracheotomy varied with the skill of the operator. The highest percentage of recoveries was that recorded by Trousseau, who reported that more than one-fourth of his cases survived—for how long a period they survived he (the speaker) did not know. He thought the high mortality in pre-antitoxin days had been exaggerated; since those days far fewer cases of the disease necessitated tracheotomy. The mortality was now from 20% to 25%.

1 *Brit. Journ. Child. Dis.*, 1929, xxvi, 200.
2 *Rev. de Méd.*, 1899, xix, 417.

Solid Œdema of One Leg: Case for Diagnosis.—D. C. HARE, C.B.E., M.D.

A. M., female, aged 38, English. Always resident in England except for short periods on the Continent.

In 1917 had an acute illness; onset with severe pain on right side of abdomen with fever (104° F.). Pain ceased after an aperient; there was some vomiting, no jaundice, no swelling of the leg. Was confined to bed for ten days. Family doctor diagnosed "liver chill."

In 1919 a second attack occurred with severe pain in the right groin, T. 104·4° F. The limb became very swollen from hip to ankle; the skin was tense and shiny, and large purple patches appeared on the thigh; movement was very painful. After about ten days swelling gradually subsided, but right leg remained slightly larger. Patient was able to dance and play hockey; she remembers one attack of "sciatica" after exertion; the swelling grew larger by imperceptible degrees. In 1925 she began to ride, and the leg then became very much worse and has continued to enlarge since. In January, 1930, during an attack of scarlet fever, the swelling almost completely subsided with the rest, but filled out again rapidly on the patient's return to walking and standing. General health very good. There were slight epileptiform attacks during adolescence but none for several years past.

On Examination.—Leg not painful; patient complains of size and weight only. It is uniformly enlarged from hip to ankle, pits very slightly on pressure, but wearing a shoe prevents swelling of the foot; skin and circulation appear to be normal; the leg is normal in colour. No enlargement of veins visible. No glands palpable. Abdominal and pelvic examination: nothing abnormal felt. A skiagram shows periosteal changes. Wassermann reaction negative. *Urine.*—No albumin.

Treatment.—Massage is said to have improved the condition temporarily, but gives no permanent improvement and has been discontinued.

Discussion.—The PRESIDENT suggested that the patient should rest until the size of the leg was much reduced, and should then firmly apply an elastic bandage from the foot upwards, and wear it continuously every day. That might maintain the reduction brought about by the rest.

Dr. BERNARD MYERS said that the condition might be due to a former inflammation in the right iliac fossa, *plus* a subsequent deposit of fat in the leg.

Dr. W. FELDMAN agreed with Dr. Myers. At the time of the illness in 1917 there might have been an inflammatory swelling in the appendix region, and there might now be adhesions which influenced the lymphatic or the venous circulation. He did not see any objection to exploration.

Dr. J. D. ROLLESTON said that Dr. Parkes Weber and he had agreed that the condition was closely allied to that in Milroy's disease, in which condition operation was not indicated. A point of resemblance was that the function of the leg was not interfered with. The patient had not been prevented from working, and she had had the condition eleven years. He would discourage operation in this case.

Dr. H. S. STANNUS said he also thought he could feel a mass on the right side of the abdomen, but it seemed to be freely movable, and he thought it was the colon. Was this mass supposed to be fixed, and was it situated where it would interfere with the circulation of the leg?

The PRESIDENT said he also had thought of the possibility of Milroy's disease, but believed that in most cases of that condition there was a family history of the disease, and also that eventually both legs became involved. In the present case, although there was no enlargement of the superficial veins, he thought that the femoral or external iliac vein was probably blocked. It was not likely that anything could be removed which would restore the circulation in that vein, and he considered that an operation was not only unnecessary, but might be dangerous. He could not detect any definite swelling in the iliac fossa, and he thought the history suggested that the patient had had acute phlebitis in 1919, and that this had been followed by thrombosis and complete closure of the vein.

Carotinæmia in a Male Diabetic. — D. C. HARE, C.B.E., M.D., and C. PILLMAN-WILLIAMS, M.B.

J. J., male, aged 48, butcher. Known to have had glycosuria since the age of 44. Two years ago treated in hospital in Wales and given insulin for a few weeks. Returned to work and remained well on a moderately restricted diet. August, 1929, gave up dieting owing to unemployment. In October, 1929, first came under our observation; he complained of loss of weight and feeling weak, and was admitted to hospital.

Family History.—Two brothers suffer from diabetes.

On examination (Royal Free Hospital, October, 1929).—General condition fair. No polyuria; no ketosis; no abnormal pigmentation noted. Diabetic condition mild, controlled with a moderate carbohydrate restriction (60 gms. CH daily) without insulin.

Recent History.—Returned to work, but received a septic scratch on his hand and was admitted to St. Pancras Hospital December 13, 1929. On admission the chief complaint was of vague pains and tingling in hands and feet. The sepsis was of a minor character, a slight abrasion and one or two whitlows; there was only one slight rise of temperature. There was no ketosis, but a good deal of glycosuria. The diabetic condition was treated by a ladder diet, and small doses of insulin were given for a short time. After six weeks he was about to be discharged when it was noted that his skin was becoming pigmented; he did not feel well, and pain developed in the left side of the chest. Since then the pigmentation has continued, though it has varied considerably from week to week without apparent cause. He does not feel well when the colour is deeper.

On March 14, 1930, he was transferred to the Royal Free Hospital.

General Examination.—Patient is red-haired with florid complexion; fairly well nourished (weight 9 st.). No fever or sweating, no abnormality found in lungs or heart; blood-pressure 114/68, liver not enlarged, spleen not palpable, no glandular enlargement. No septic focus.

Skin.—Texture normal; when well pigmented there is a uniform light-yellow tinge over whole body, intensified over extremities and face, and becoming bright "canary-yellow" on palmar and plantar surfaces. On forehead and bald part of scalp colour is brownish-yellow; is less intense on cheeks and neck. Sclerotics faintly yellowish; mucous membranes appear normal.

Blood-serum extract shows a definite yellow pigmentation of the colour of potassium bichromate solution. The van den Bergh serum reactions, direct and indirect, are consistently negative.

Urine Pigments.—Traces of urobilin are present; no bile-pigments found on any examination.

Liver Efficiency Tests.—Lævulose sugar meal shows a blood-sugar rise of 60 mgms. (0·098 to 0·160 grm. per cent.), indicating a severe deficiency.

Blood- and Urine-Sugar Estimations.—Resting blood-sugar always normal (0·09 to 0·1 gm.). 25 gms. glucose on October 25, 1929, gave an almost normal curve with a maximum of 0·185 and a trace of urine-sugar. With 100 gms. glucose the curve was of diabetic type with a delayed rise to 0·255, and a delayed fall and 2·8% urine-sugar. On daily intake of 50 gms. carbohydrate, sugar is occasionally present in the urine, and acetone tests are negative.

Diet with regard to Carotin Content.—From December 13, 1929, to March 15, 1930, the daily diet included 3 eggs and 10 to 16 oz. cabbage, no other vegetable was taken. Pigmentation developed about the beginning of February. Since March 17 the daily diet has included 2 eggs, 8 oz. cabbage, 4 oz. tomato or lettuce.

The results of administering pure carotin in daily doses of 10 mgm. over a

period of ten days were as follows: *First estimation.*—Carotin = 0·39 mgm. per 100 c.c. blood. *After carotin had been given for three days.*—Carotin = 1·19 mgm. per 100 c.c. *After nine days' administration.*—Carotin = 1·2 mgm. per 100 c.c. The test had also been made on another patient, a youth, aged 19, who had severe diabetes. The latter showed no pigment changes in the serum before or during the test.

(We are indebted to Dr. Feldman, of St. Pancras Hospital, for facilities for seeing the patient while there, and to Dr. Shaw for an account of the treatment. A second case of carotinæmia in a male diabetic was demonstrated at the meeting by kind permission of Dr. Feldman.)

Comments.—In a paper published by Dr. Stannus [1] last year there is an excellent historical survey of the condition and a valuable bibliography with an account of a case associated with myxœdema. Dr. Rabinovitch [2] in 1928 made a study of 1,014 diabetics, and in only 59 found any suggestion of excess of carotin in the blood. In that number the percentage of severe diabetics was higher than in the whole number, but carotinæmia could not be related to a specially high vegetable intake. In 11 of the cases there was a positive van den Bergh reaction, that might be related to the hepatic condition.

The present case is not one of severe diabetes, but there is a definite liver deficiency that has impressed us as a possible factor in the production of the carotinæmia. (Curves shown and demonstrated.)

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Discussion.—Dr. H. Stannus said that most of the cases of the kind under discussion were in diabetics with ketosis. His own case was one of hypothyroidism, a case, in fact, of myxœdema. As the myxœdema cleared up with the giving of thyroid, so the pigmentation disappeared. He discontinued the thyroid treatment, and the myxœdema and pigmentation returned, but cleared up again with permanent dosage with thyroid. Carotin was taken in food substances, and was a normal constituent of the body.

The point at issue in this and similar cases was, why there occurred a hyper-carotinæmia and a deposition of carotin in various tissues. The only common factor he could find was a hypo-oxidation in the body. In Dr. Hare's case there was defective liver function, and this might account for the condition, the excess of carotin in the tissues occurring as the result of its not being taken up by the liver and changed into vitamin A, as Moore had shown to take place normally. Milder cases were not, he thought, detected, but they would be evident if looked for.

Dr. Harold Avery said that carotinæmia seemed to be more common in diabetics than was generally supposed, and rather more common than the 5·9% which Dr. Rabinovitch suggested. A yellow pigmentation of the palms and soles was common, but it might be that the pigment was fixed in the tissues and did not so often circulate as such in the blood, but in some allied form, perhaps as vitamin A.

The President asked whether Dr. Hare considered that the condition had any clinical significance, especially when associated with diabetes, with regard either to prognosis or to treatment.

Dr. D. C. Hare (in reply) said she was unable to answer the President; her experience was confined to one case of the kind. The literature showed a very slight correlation between the severity of the diabetes and the condition of carotinæmia. In a much more severe case of diabetes which she had tested with daily doses of carotin, there was no evidence of carotinæmia whatever, judged by the extract of the blood-serum. She agreed with Dr. Avery that a slight degree of carotinæmia might be common, as one might easily fail to recognize it by the colour of the skin.

Post-mortem Specimen from Case of Aneurysmal Dilatation of the Left Auricle (shown at the Meeting held on March 14, 1930[1]).—MAURICE A. CASSIDY, C.B., M.D.

This patient was a married woman, aged 35. She had had rheumatic fever twice, and had double mitral disease and auricular fibrillation for many years. While she was under observation a large dull pulsating area developed to the right of the sternum, and over this area a mitral systolic murmur could be heard, and the thrill due to it was felt. Radiographically, there was seen an enormous cardiac shadow,

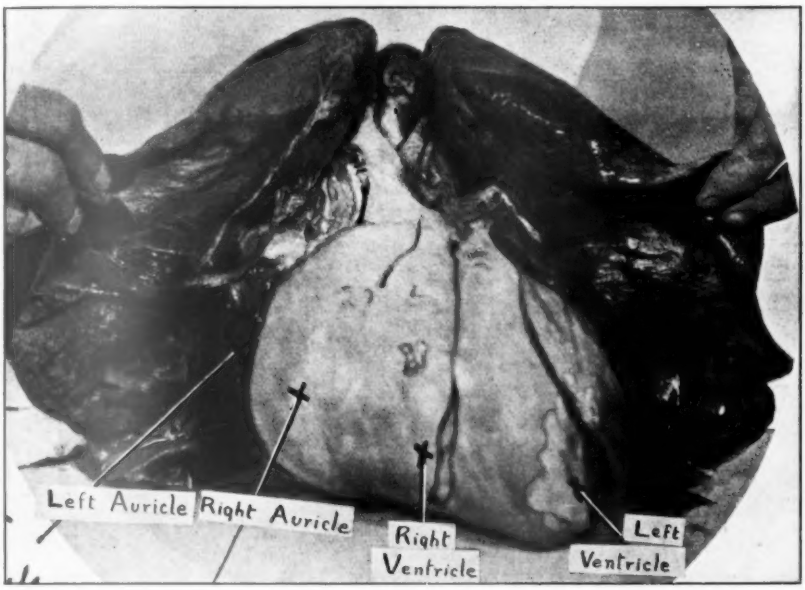

FIG. 1.—Aneurysmal dilatation of the left auricle: view from in front and below. (Dr. Maurice Cassidy's case)

in shape like a Rugby football, and there was an acute cardio-phrenic angle. There was no engorgement of the veins of the neck. The patient died a few weeks after being shown here, cardiac failure having suddenly developed.

The specimen shows compression of the œsophagus—to a mere ribbon—which caused dysphagia, and a widening out of the bifurcation of the trachea, so that the two bronchi are in a straight line.

[1] *Proceedings*, 1930, xxiii, 955 (Clin. Sect., 71).

The left auricle is now about half the size it was before being put into formalin, but is still as large as the whole of the rest of the heart. The pericardium was closely adherent to the left auricle posteriorily. The mitral valve admitted three

FIG. 2.—Aneurysmal dilatation of the left auricle: view from the right side.
(Dr. Cassidy's case.)

fingers; it was grossly thickened and calcified. The wall of the left auricle was very thin, and on microscopical examination was seen to be composed almost entirely of fibrous tissue, only a few scattered muscle fibres being found.

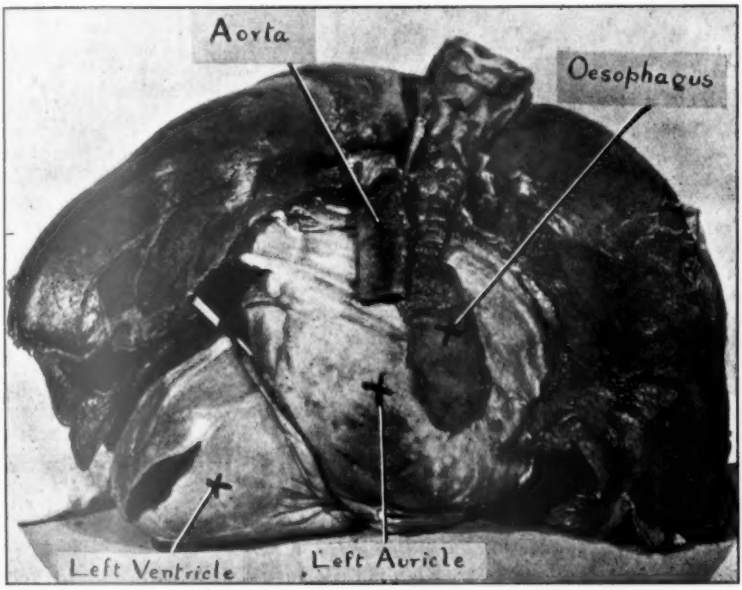

FIG. 3.—Aneurysmal dilatation of the left auricle: view from behind. (Dr. Cassidy's case.)

Hæmophilia treated by Liver Diet.—H. V. MORLOCK, M.D.

Patient, a boy, aged 15, has been known to be a hæmophiliac for eleven years. Has had repeated effusions into ankle, knee and wrist-joints. Admitted to hospital on this occasion for abdominal pain, hæmaturia and effusion into both ankles (three weeks' duration).

No family history of hæmophilia obtained.

Condition on Admission.—Pale, thin, undersized; marked limitation of movement in both knee- and ankle-joints. Ankle-joints swollen and discoloured. Frank hæmaturia.

December 30, 1929: R.C., 2,570,000; Hb., 28%; C.I., 0·56; W.C., 23,500. *Differential count:* Nothing abnormal discovered. Blood coagulation time, 10 min. 40 sec.; control 3 min. 5 sec.

December 31, 1929: Blood transfusion (300 c.c.). January 1, 1930: Blood coagulation time, 9 min. 50 sec. January 7, 1930: 10 min. 30 sec. January 8, 1930: liver extract (= ½ lb. liver) daily. January 14, 1930: No R.B.C. in urine. January 17, 1930: Blood coagulation time, 12 min. 30 sec. January 27, 1930: Blood coagulation time, 7 min. 35 sec. February 5, 1930: Blood coagulation time, 8 min. 30 sec. February 14, 1930: Tonsillitis, pyrexia, hæmorrhage into wrist-joints; blood coagulation time, 14 min. 10 sec. February 26, 1930: Blood coagulation time, 13 min. 10 sec. February 27, 1930: Liver extract stopped. March 4, 1930: Blood coagulation time, 8 min. 10 sec. March 11, 1930: Blood coagulation time, 15 min., ankles and wrists swollen. March 12, 1930: Nateina (20 tablets daily). March 22, 1930: Blood coagulation time, 15 min. April 2, 1930: Blood coagulation time, 15 min. April 3, 1930: Nateina stopped. April 9, 1930: Blood coagulation time, 15 min. April 10, 1930: Liver extract (= ½ lb. liver) and ½ lb. liver on alternate days. April 16, 1930: Blood coagulation time, 12 min. 10 sec. April 24, 1930: Blood coagulation time, 11 min. 20 sec. May 1, 1930: Blood coagulation time, 15 min.

Discussion.—Dr. PHILIP ELLMAN asked whether this was regarded as a definite case of hæmophilia, and whether a blood-platelet count had been taken. It would have been interesting to see a skiagram of the affected joints in this case.

Dr. MORLOCK (in reply) said that the number of blood-platelets were 200,000. He regarded the case as one of hæmophilia because of the prolonged coagulation time. He was unable to get a family history of the condition, but that did not exclude the diagnosis. The patient bled for no apparent reason, or after only a very slight trauma.

Congenital Hæmolytic Anæmia, with Splenomegaly, in a Child: Controlled by Liver Treatment.—E. C. WARNER, M.D.

Sylvia K., first seen August, 1928, at the age of 1 year and 8 months, on account of severe anæmia and jaundice.

Family History.—No family history of any similar condition. One other child, quite healthy. Patient's mother was rather anæmic and said she had always been so; her hæmoglobin was 74%; red and white cell counts and fragility of red cells, normal. On physical examination of the mother nothing abnormal was found; there was no splenomegaly.

The patient was breast-fed until 7 months old. Bronchopneumonia at 17 months. Always had adequate mixed diet. Present illness dated from birth. She had always been pale, but especially so since age of 1 year and 6 months. At times was rather yellow, but never deeply jaundiced. Bowels were open regularly; fæces sometimes rather pale in colour, but never white.

Admitted to the Miller General Hospital, August, 1928, evidently very anæmic. Pale yellow pigmentation of skin and (slightly) of the sclerotics. Weight 25 lb. Liver of normal size and consistency, but spleen enlarged and rather firm, extending to level of umbilicus. The fæces were normal in colour, and the urine contained no bile-salts or bile-pigments.

Blood-count: R.C., 2,060,000; Hb., 32%; C.I., 0·75; W.C., 9,100. *Differential*: Polys., 51·5%; lymphos., 45·5%; large hyals., 3·0%.

Blood-film showed some polychromasia and anisocytosis, but no nucleated red cells or abnormal white cells, and mean diameter of red cells was normal. Fragility of red blood-cells in saline solution was normal. Wassermann reaction (blood), negative; urobilin in urine strongly positive. Van den Bergh reaction in serum, negative direct and strongly positive indirect. Fæces showed no occult blood or abnormal micro-organisms.

The patient was treated with iron ammonium citrate, gr. 5 t.d.s. until November, 1928, when the hæmoglobin rose to 60%. She then had a severe hæmolytic crisis, with a temperature of 102° and became very anæmic; skin and sclerotics were tinted pale yellow. Hæmoglobin, November 3, 1928, was 20%, and blood contained many nucleated red cells. On November 6 hæmoglobin was 18%.

Three transfusions of blood—total 285 c.c.—were carried out between November 6 and November 12, on which date the hæmoglobin was 24%.

The red cells were still being rapidly hæmolysed on November 7, when treatment with liver extract, equivalent to ¼ lb. fresh liver, was begun. On December 3 hæmoglobin was 34%, but patient was still rather yellow. Then ¼ lb. fresh liver was given in addition. On December 26 hæmoglobin was 42%, and on February 12, 1929 (date of discharge from hospital), the hæmoglobin was 66%.

The yellow pigmentation had disappeared and there had been no hæmolytic crisis since the treatment by liver extract was begun. In March, 1929, she became slightly yellow when she had whooping-cough. Otherwise she has remained perfectly well, and is taking the same quantity of liver regularly.

On re-examination, January 27, 1930, at the age of three years and two months, she looked extremely healthy, and had gained 8½ lb. in weight. Abdominal examination showed liver normal and spleen decreased in size so that it was just palpable at the end of a deep breath. Blood re-examined by Dr. H. F. Brewer who reported: "Hb., 72%; R.C., 5,120,000; C.I., 0·70; W.C., 10,600. Polys., 55%; lymphos., 40%; large monos., 4%; eos., 1%. Anisocytosis and poikilocytosis are within normal limits; no polychromasia, punctate basophilia or immature white cells. Van den Bergh reaction, direct negative; indirect less than 0·5 units."

The dose of liver has gradually been reduced, so that now she has only the equivalent of ¼ lb. per day, and the hæmoglobin value is keeping steady at 72%.

The essential features of this case are a severe hæmolytic jaundice, not due to Addison's anæmia or to acholuric jaundice, occurring in a child one year old, with an enlarged spleen reaching to the level of the umbilicus. This condition has been controlled by liver treatment, and, at the end of nineteen months the spleen is only just palpable. The dose of liver necessary to control the abnormal hæmolysis was equivalent to ½ lb. of fresh liver. On half this dose, the hæmoglobin was rising very slowly, and the yellow pigmentation of the skin persisted. The full quantity, which is the full dose normally given to an adult, has been taken without any ill-effect for fifteen months, and the hæmoglobin has risen to a normal value of 72%. The quantity of liver has gradually been reduced to ¼ lb. daily, and the patient, so far, has shown no signs of increasing hæmolysis.

Discussion.—Dr. NORMAN HILL said that he had had a somewhat similar case of congenital anæmia in an infant. The child had been pale and the red cell-count had been under two millions. After perseverance with liver treatment the blood became normal. The infant remained well six months without the further use of liver. It was one of twins, and the other infant furnished a control as to the feeding. In cases of ordinary secondary anæmia in adults he (Dr. Hill) had been disappointed with the result of liver treatment.

Dr. W. M. FELDMAN said that he had had a case of myelogenous leukæmia in which the patient had died a fortnight ago. He was becoming practically all right under X-ray treatment until, as a result of a cab accident about eighteen months ago, he nearly bled to death. On being admitted to University College Hospital he was transfused, and since that time began to go downhill. Liver extract was tried, but it had no effect on the myelogenous leukæmia.

Dr. HAROLD AVERY said that in a case which he had recently described of anæmia of the pernicious type, following polycythæmia, liver treatment had produced an increase in the red cells. And in a case of puerperal anæmia of the pernicious type there was great improvement on liver treatment; but one could not say how soon the patient in this latter case would have recovered without that treatment, as she was also having iron and arsenic.

Dr. WARNER (in reply) said it seemed from observation of a number of cases that in those with increased hæmolysis there was benefit from liver therapy, but when this feature was absent the cases did not improve with liver treatment.

Transposition of the Viscera.—HAROLD AVERY, M.B.

C. N., male, aged 31. The condition was found during routine examination The transposition affects all the viscera of the chest and abdomen. It can be demonstrated by physical examination and radiography. No physical deformities are present.

Discussion.—DR. W. M. FELDMAN said that a few years ago he had published a case of complete transposition of viscera in a baby three weeks old. The infant was brought on account of bronchial trouble, and ultimately died. No autopsy was allowed. The electrocardiogram showed that the T-wave was normal for the left heart, but abnormal for the right, though the heart was situated on the right side. It would have been interesting to see an electrocardiogram of the present case. In the case he referred to it was suggested that the inversion of the T-wave indicated some degeneration of the muscles of the ventricle, and that this might have caused death.

DR. AVERY (in reply) said that this patient was perfectly fit in every way; he was in the Army during the war. It was interesting to note that the man was right-handed, so that his cerebral hemispheres were not transposed.

Occipital Tumour.—W. M. FELDMAN, M.D.

F. T., female, aged 19.

History of mild fits which began ten years ago. These were of short duration, and mainly took the form of a sudden dulling of perception with mistiness of vision. They diminished greatly in frequency under treatment with bromides and other

sedatives. Their character became more severe in 1929, when consciousness was lost for the first time during an attack, and vomiting occurred. Patient became aware of limitation of vision in December, 1929. Has since then had attacks of vomiting.

Condition on Examination, before Operation.—Visual field: left homonymous hemianopia; bilateral papillœdema with white areas in left disc. Weakness of right lower face and decreased motor power. Right arm: tendon-jerks more active than left. Right leg: motor power less than left. Right knee- and ankle-jerks greater than left. Right plantar reflex, just flexor; left, definitely so. Sensations normal. Mentality normal.

Decompression Operation, March 6, 1930 (Mr. F. McG. Loughnane).—Right occipital tumour discovered. Portion of occipital bone (right side) trephined and removed. Brain tissue bulged through opening after reflection of dura. Occipital lobe incised and 10-15 c.c. yellowish fluid escaped under pressure.

Sequelæ.—No pressure symptoms (headache, vomiting). Visual fields contracted and patient became totally blind within a week after operation. Double optic atrophy now present. Definite Babinski response on right side, doubtful on left side (April 11, 1930). General health fair.

A portion of what was thought to be cyst wall was taken out. The slide does not show a cyst wall, but only grey substance, with much hæmorrhage in it.

When being brought here to the meeting the patient had an attack of vomiting in the car.

Hernia of the Lung.—CECIL P. G. WAKELEY, F.R.C.S.

J. S., male, aged 63, has complained for the last two years of a swelling in the region of the right supra-clavicular joint. He was quite well until he first noticed the swelling. Soon after the swelling appeared, pulmonary tuberculosis was diagnosed, and he has been under treatment in a sanatorium. During treatment the swelling in the region of the sterno-clavicular joint has varied in size from that of a tennis ball to that of a walnut. At times it almost disappears, while at others it passes up into the neck. When the patient has an attack of coughing the swelling may be as large as an orange. There is constant pain in the region of the sterno-clavicular joint.

On examination, there is a fluctuating swelling which passes behind the sterno-clavicular joint, and which gives rise to an expansile impulse on coughing.

The vocal fremitus is diminished on the right side of the chest, and voice sounds are increased. Skiagrams reveal caseous tuberculosis at the right apex. Wassermann reaction negative.

Discussion.—The PRESIDENT said he agreed with the diagnosis; he had never seen a similar case. He would treat it simply by pressure with a pad.

Dr. PHILIP ELLMAN said he had had to deal with a large number of cases of early and advanced pulmonary tuberculosis, but could not recall having seen such a complication. Patients suffering from tuberculosis strained themselves a good deal in coughing, and one would expect such an accident more frequently, especially as there was a thin piece of fascia—Sibson's fascia—in relation to the apex of the lung. In this case he (the speaker) could feel a swelling after the patient coughed, and it felt very crepitant, as though it might be lung. If it were a hernia of the lung, as was most likely, treatment by apicolysis might produce a good result. Surgical emphysema had to be considered as a possible diagnosis.

Dr. NORMAN HILL agreed with the diagnosis and with the President's recommendation to treat by pressure pad. He would not advise surgical interference.

Mr. CECIL WAKELEY (in reply) said he did not think this was a case of surgical emphysema; had it been so, the emphysema would have been rapidly absorbed, and there would not have been a definite localized swelling. He agreed that it was not worth while to attempt anything operative, and this was also the view of his colleague, Dr. East.

Section of Neurology.

[*March* 13, 1930.]

DISCUSSION ON INJURIES TO THE BRACHIAL PLEXUS.

Dr. Wilfred Harris: Injuries of the brachial plexus may be divided into: (1) injuries involving the primary nerves—often spoken of wrongly as "roots"—near the foramina; (2) lesions of one or more of the three cords of the plexus, or of branches to these cords from the primary nerves; and (3) lesions of the main nerves to the upper extremity, after their emergence from the plexus itself.

The primitive type of plexus consists of two cords only, an anterior and a posterior, such as is seen in birds and reptiles, and scarcely any communication exists between these two systems once the primary nerves have formed these two cords. The brachial plexus of Man and of the other Mammalia, with the possible exception of the Monotremata, Echidna and Ornithorhynchus, may be considered to be of the same type, the inner and outer cords together being equivalent to the anterior cord of the more primitive animals.

One of the common causes of injury of the nerve trunks just below the plexus is downward dislocation of the shoulder. This may cause paralysis of the musculo-spiral, median, ulnar, or musculo-cutaneous nerves, by means of pressure of the head of the humerus in the axilla. An even commoner paralysis following dislocation is of the deltoid, due to the downward dislocation of the humerus pulling on the circumflex nerve, as it winds round the neck of the bone. At the same time the infraspinatus muscle is torn away from the branch of the suprascapular nerve, which winds under the acromion to enter the under-surface of the muscle. Such paralysis of the deltoid and infraspinatus resembles the arthritic atrophy of these muscles found in tuberculosis or osteo-arthritis of the shoulder-joint, except in the electrical changes. Similar localized atrophy may also be met with in partial Erb's palsy, and in acute poliomyelitis.

Very severe plexus damage, in which all the trunks are torn near their exit from the foramina, may result from accidents such as a bicycle crash between two meeting tramcars, or from being caught up in revolving machinery. Partial plexus injuries of the primary nerves may involve the lower "roots" or the upper.

When the lower two nerves, eighth cervical and first dorsal, which unite to form the inner cord, are damaged, the paralysis is called "Klumpke palsy." This involves the intrinsic muscles of the hand and the flexors of the fingers. If the first dorsal nerve is damaged close to the foramen, the branch to the cervical sympathetic will suffer, causing slight ptosis and a small pupil that does not dilate well when shaded. Lesions of the uppermost "roots," the fifth and sixth cervical, are known as Erb's palsy, or Erb-Duchenne paralysis, the latter observer having described this paralysis in new-born infants, from traction on the neck. The fifth is the highest cervical nerve taking any large share in the plexus, and, being more inclined downwards, it

will be more liable to be stretched and damaged from falls on the point of the shoulder, when the arm is not put out to save the blow. The head is then violently wrenched in the opposite direction, thus stretching the nerves to the plexus, and the fifth nerve, being the highest and most liable to suffer, is often ruptured within its sheath at its junction with the sixth. In the high or prefixed type of plexus this may result in total palsy of the deltoid and spinati, all the flexors of the forearm, the radial extensors of the wrist and the pronator radii teres.

In the low or post-fixed type of plexus, only the deltoid and spinati will be completely paralysed, flexion of the forearm being only weakened. No anæsthesia is ever found with Erb's palsy, unless the sixth nerve is also damaged. The serratus magnus, whose main branch to the long thoracic nerve is given off close to the foramen, is never affected.

Stab or gunshot wounds above the clavicle may injure the plexus itself, one or more of the three main cords—inner, outer, or posterior—or branches to them from the primary nerves. Suppurating wounds in this region are followed by such dense fibrous tissue surrounding the nerve trunks as to make operative assistance quite hopeless.

Careful analysis of the muscular paralysis and of the anæsthesia present, if any, will generally lead to an accurate localization of the injury. For example, in a certain case, slight flexor weakness of the fingers was associated with complete palsy of the flexor carpi radialis, but not of the flexor carpi ulnaris, or of any of the intrinsic muscles of the hand, while well-marked partial median anæsthesia of the hand was present. Since the thenar eminence gets its motor supply through the inner cord, the inner head of the median nerve could not have been damaged, but the sensory loss indicated the outer head of the median as being damaged, as we know the sixth and seventh nerves supply sensation to the thumb and outer two fingers, and the flexor carpi radialis has a higher root supply than that of the flexors of the fingers.

Mr. Geoffrey Jefferson : Two points are of the greatest importance in brachial plexus injuries : the site of the lesion and its precise nature. The mechanisms of birth injury and of rupture later in life are the same, the lacerations being caused by forcible separation of the head and shoulder, with depression of the shoulder as the essential factor.

I shall confine myself to the injuries of the roots and cords, the so-called supraclavicular type, since these are, in the main, the pure injuries of the plexus uncomplicated by fracture. Duchenne first accurately described the individual muscle palsies of birth injury without hazarding an explanation ; Erb suggested that the pressure of the obstetrician's finger on the point of junction of the roots C5 and C6 was the cause of this disablement. Fieux was the first to suggest that traction was the cause of the rupture, and that the roots of the plexus tore from above downwards in such a manner that C6 would not give way till C5 had gone, that C7 would follow C6, and so on from above downward. These ideas have dominated the theory and practice of the surgery of brachial plexus injuries down to this day and it is important that we examine them more closely. Fieux cut the dissected roots of the plexus on the cadaver, and then, after abducting the head and neck as far as possible, measured the distances which separated the cut ends. This was greatest in the case of C5 and C6, and least in T1, a perfectly accurate observation. But the outstanding impression that one draws from his diagrams is that the plexus tears through its roots extra-vertebrally at the same relative place all the way down, leaving in the neck an accessible stump which could be sutured to the distal portion. This conception must be modified, for experience has taught us that there is no absolute uniformity in the site of the lesion or in its minute character, the tear tending to be ragged and " stepped."

It is true that in the common traction injury C5 usually tears first, and that this tear is followed or accompanied by one in C6. C7 often gives way before the injury to C6 is complete, and the injury to C7 may be more severe and more deeply situated than the injury to the upper cords. The same may happen to C6 above, to C8 below, and, rarely, to T1. Thus the site of rupture is not the same in all the roots of the injured plexus, but may be at different depths; some are torn extra-vertebrally, some intradurally. My amendment can be stated in graphic

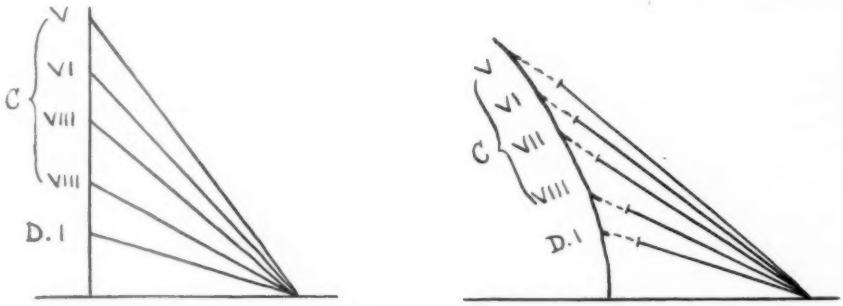

FIG. 1.—Fieux's diagrams illustrating the mechanism of brachial plexus injury.

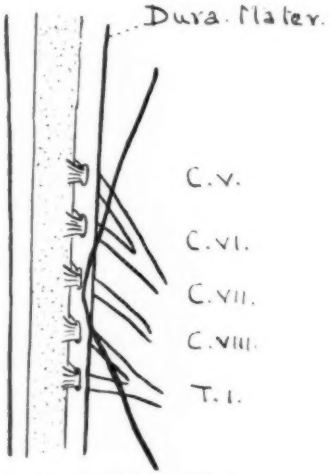

FIG. 2.—Graph showing the depth-incidence of injury in traction lesions of the brachial plexus.

form (fig. 2). It is based on the work of Adson and of Pope and on anatomical and clinical investigations. The curve of the site of rupture goes deeper as we descend to C7, and then again becomes more superficial. The reason for the severity of the injury to C7 is that it is the shortest branch of the plexus and begins to feel the strain before the rupture of C5 and C6 is complete, whilst the reason for the complete withdrawal of this root is probably to be found in its more horizontal direction. This allows of its being pulled more directly out of its inter-

vertebral foramen. Injury to C5 and C6 may also be intradural, but not so often as in the case of C7. The great length of T1 saves it often from withdrawal, whilst C8 occupies an intermediate position. Intradural rupture of C7 is not constant, for any of the upper roots may be ruptured deeply, but the diagram illustrates a distinct tendency in plexus injuries. As to intradural rupture, it is likely that the site of rupture is at some small distance from the cord, probably just proximal to the fused anterior and posterior roots and ganglion. It is uncommon to find signs of cord damage; it has not been present in any of my own cases. The presence of blood in the cerebrospinal fluid, to which Sharpe drew attention, is of no significance on this point. One may compare this tearing of roots with avulsion of the sensory root of the trigeminal nerve for neuralgia, a procedure theoretically dangerous, but practically benign; the cord is so well supported by tough pia that the more friable nerve fibrils would give first. One further point arises out of these considerations, and that is the severity of pain after brachial plexus injuries. In some, pain is early and distressing; when severe it denotes a root lesion. Its distribution may also help us to gauge which roots have been injured, and it is, in general, of great prognostic value, for these cases with very severe root pains do not recover function. In one case seen recently pain has been present for over twenty years, and the root-tearing was confirmed at operation on the cord.

Case I.—J. B., male, aged 46. In 1908 his right arm was caught in machinery and violently pulled. Complete paralysis and anæsthesia followed. Horner's syndrome present. Arm amputated eighteen months later by Dr. Harris at Rochdale. Good stump. Considerable pain, especially on the back of the phantom wrist and hand. In 1911 the late Sir William Thorburn divided the brachial plexus close to the intervertebral foramina. No relief. Six months later he performed a laminectomy, but found the dura adherent to the right side of the spinal cord. It was impossible to make a clean posterior root division, owing to the obliteration of normal anatomical structure. Seen by the writer in 1930: pain is still troublesome.

Clinically, there are other points which are helpful in allowing us to define the exact point of section of the nerve trunks. In general, the more roots involved, the deeper will be the level of some of these lacerations. Very great violence is needed to rupture the plexus—as everyone who has attempted to produce plexus lesions on the cadaver will admit—and has undoubtedly been applied in many of the cases which we see clinically. The injuries which follow the modern motor accidents tend to be of very severe type. There are three branches of the plexus which help us to diagnose the level of the injury; those which come off very early from the roots—the nerve to the rhomboids, the nerve of Bell (posterior thoracic nerve) and the sympathetic. The presence or absence of paralysis in the rhomboids and serratus magnus gives us valuable information as to the point of damage to C5 and C6, whilst the presence or absence of Horner's syndrome instructs us concerning T1. Signs of injury to the cervical sympathetic occurred in only one of my own cases, and I think it is uncommon.

We are left with nothing to guide us as to the site of rupture in C7 and C8, except pain in their root areas and the probabilities of the case, as assessed on the extent of the damage to the whole plexus. This statement alludes to the roots of the plexus themselves. If the injury is in the cords or trunks, then the points to which Dr. Wilfred Harris has referred (dissociation of motor and sensory paralysis in the hand-distribution of the median nerve) will be of great service.

Another point which may be useful is the presence of a small patch of anæsthesia on the back of the shoulder, high up. This occurs in ruptures when the injury is at the level of the intervertebral notch, or deeper, that is, before the posterior primary division has come off. But, if limited to a single trunk, such as C7 or C6, overlap will probably make the loss difficult to distinguish.

In other instances the classical mechanism results in an injury further away from the cord. Variations in muscular development, in the direction of the force, and in the posture of the patient, probably account for these divergences. Sometimes the great vessels may also be coincidently injured.

Case II.—J. S., aged 24. In October, 1925, a steel plate slipped from an overhead crane and fell on to his left shoulder. He was unconscious for a short time. On recovering he found his left arm paralysed and numb. A large hæmatoma formed over the root and side of the neck, and upper part of chest and shoulder. Dr. Mandall Coates, of Horwich, could find no radial or brachial pulse. Palsy of muscles of shoulder-girdle and upper arm. Gradual recovery. For six weeks the hand was very cold. Seen by the speaker seven weeks after the accident, intervention not advised. Now (in 1930) patient is employed in railway works and has excellent function in the arm in spite of complete wasting of the deltoid, biceps, brachialis anticus, and clavicular head of pectoralis major, and weakness in triceps. The brachio-radialis is acting strongly, giving powerful flexion of forearm; the spinati act well and abduct the shoulder. A severe muscle palsy of limited distribution with excellent functional result, hand and forearm normal save for the thready pulse. Apparently an injury to the third part of the subclavian or origin of axillary artery, as well as a tear of the outer cord of the plexus below the supra-scapular nerve.

The Nature of the Injury.—The injury to the nerves is very rarely a direct clean snapping across of the roots or trunks of the plexus. It is quite clear that in a structure of such unequal tensile strength as a large plexus, the points of rupture of the constituent parts will vary and the damage will be spread over a wide area. This is a commonplace of observation at operation when nerve callus, as Cone so aptly termed it, is so dense as to convert the plexus into a bewildering mass, incapable of resolution by dissection into its normal parts. It is not uncommon to find the scar spread over a length of several centimetres. This brings us to the treatment, which must be based on the pathological anatomy of the lesion. It is more or less tacitly accepted that the results of suture of injuries of the peripheral nerves are very good, and one might infer that the results of operation on the injured plexus might well be equally satisfactory. It is worth saying that the results of peripheral nerve suture are not nearly so good, judged neurologically, as is generally believed, though a fairly useful limb generally results. The reasons for such disability as follows are two—first, the interference of scar tissue, and secondly, "vicious" axonal permeation. In the brachial plexus the second factor only too rarely has a chance to come into play, fibrosis effecting a barrier to regeneration. If the views which I have put forward as to the nature and site of the injury in plexus injury are accepted, it follows that many injuries, especially the severe ones, in which one most wishes to give some help, are not now, and never will be, amenable to surgery, some because of the depth of the rupture, others because they are not clean ruptures, but endo-neural dissolutions. The latter type is most often encountered in the more peripheral injuries, those of the trunks and cords, where the fraying out of the nerve fascicles within their sheaths presents the surgeon with an insoluble problem. I do not believe, in spite of the occasional good result which appears in the literature, that secondary suture of the brachial plexus is a possible operation, except on rare occasions. An important principle in the secondary suture of peripheral nerves is the mobilization of the trunk, and this is impossible in the case of the brachial plexus.

I should like to lay it down for purposes of discussion that, whilst immediate primary suture may sometimes be feasible in those uncommon cases where two clean nerve-ends are discoverable in an accessible position, secondary suture should never be undertaken. One should operate for plexus injury within a week or ten days or not at all. The exact nature of the lesions can then be discovered once and for all, and the future of the patient assessed with greater clarity. No doubt, even then, disappointment will be the general rule, but the surgeon will at this stage be spared the temptation of interfering with fibres which may be regenerating, always a danger at a later date.

Mr. Blundell Bankart said that he had never seen useful recovery follow resection and suture of any considerable part of the brachial plexus after a traction injury; whether the plexus was operated upon or not, the treatment of these cases was essentially orthopædic. From an orthopædic point of view, a nerve injury was considered to be recoverable until the contrary was proved by time, and time was the only thing which would settle this point, although in a particular case one might judge from the severity of the injury that ultimate recovery was extremely improbable.

Treatment did not influence the recovery of a nerve, which either recovered or not according to the amount of damage done to it. Orthopædic treatment was concerned almost entirely with the care of the paralysed muscles during the period during which recovery might be expected. It was proved beyond doubt that passive stretching of paralysed muscles prevented or delayed their recovery, and the first principle in treatment was to secure continuous relaxation of the paralysed muscles during this period. In the case of a plexus injury, an abduction splint or plaster to keep the arm abducted to a right angle, the elbow also at a right angle, and the wrist (if affected) dorsiflexed, was now standardized orthopædic treatment. The disadvantage of plaster was its weight; its advantage was that it prevented unnecessary and harmful interference with the paralysed muscles. He was convinced that such harm was done by indiscriminate massage and movements in these cases. When a muscle had recovered its innervation and was able to contract, however feebly, it should then be encouraged to contract and develop its strength, and the best substitute for natural contraction was electrical stimulation. Neither this nor any other treatment should be prolonged indefinitely, certainly not for more than two years, and only for that time if definite recovery continued to be observed. In some cases of total plexus paralysis the lower roots recovered rapidly, leaving a residual upper arm paralysis. He believed that lower root paralysis recovered very rapidly or not at all, so that it was useless to spend months of treatment on a residual paralysis of the hand. When the hand was spared or had recovered, something might be done to improve the movements of the arm. Transplantation of muscles, e.g. trapezius into deltoid, was useless. On the other hand, arthrodesis of the shoulder-joint was a very useful operation when the scapular muscles were active, and by this means the usefulness of the whole limb could be very much improved.

Dr. Henry Cohen: Cases of so-called upper cord lesions (C5 especially) may occur when the arms are bent upwards and backwards at the shoulder and the fingers of both hands locked behind the neck during anæsthesia. It has been suggested that in this way the fifth and sixth cervical nerves are compressed between the clavicle and the transverse processes of the spinal column, or alternatively that the compression takes place between the clavicle and the first rib. If, however, it is remembered that there is an anterior convexity in the cervical spine, the traction hypothesis which Fieux applied to the usual type of Erb-Duchenne paralysis is the most satisfactory explanation of this increased type. He (the speaker) would emphasize the necessity for radiological examination in all cases of injury to the brachial plexus. Subluxations and fracture-dislocations of the cervical spine not infrequently accompanied the nerve injuries; he had known a case in which injury to the lower cord of the plexus had resulted from ligature of the subclavian artery.

Dr. Russell Brain described a patient who was found to have a lesion of the brachial plexus on awakening one morning; he was in the habit of sleeping with the affected arm abducted and externally rotated. He (the speaker) suggested that in that case pressure or traction on the plexus had been responsible for the injury. He

also suggested that a similar cause might operate in the production of some cases of so-called "radiculitis," which so frequently involved the fifth cervical root. He emphasized the importance of examining the arteries of the upper limb in suspected cases of brachial plexus injury, and he described a case sent to him with a diagnosis of crutch palsy, but in which the symptoms were due to obstruction of both axillary arteries. Tourniquet palsy was a condition which, in the absence of a good history, might be confused with a lesion of the brachial plexus.

FURTHER ILLUSTRATIVE CASES.

Injury of Brachial Plexus.—J. PURDON MARTIN, M.D.

Geoffrey M., now aged 12, knocked down by a motor-car August, 1927: left clavicle broken. Fracture was reduced and left arm fixed with strapping in usual position across chest. When strapping was removed three weeks later, arm was found to be severely paralysed. Skiagram showed clavicle in excellent position and did not indicate any excess of callus, so that paralysis was probably due to injuries sustained at time of accident and not to involvement of nerve trunks in callus. Condition was as follows: Deltoid acted very feebly; triceps had good power but was not normal; paralysis was all but complete in biceps; below elbow all muscles were completely paralysed. Sensation completely lost on radial side of forearm and over whole hand; on rest of forearm loss not complete, pressure and strong painful stimuli being appreciated; on median half, loss less severe near elbow than towards the wrist.

From the presence of some voluntary contraction in the triceps and biceps, and of sensory appreciation in the area supplied by the median cutaneous nerve of forearm, it was evident that none of the cords of the plexus was divided, and it seemed more likely that the lesion affected their terminal branches. There was evidence also that the circumflex, musculo-spiral, musculo-cutaneous and internal cutaneous nerves were not completely interrupted, and soon there was similar evidence in regard to the ulnar, for the patient began to appreciate pressure on the ulnar side of the hand; in the median nerve, paralysis was complete. The arm was put up in the abducted position and daily massage and electrical treatment were given.

Six months afterwards considerable recovery had occurred in the deltoid and biceps, though in the biceps there was not yet sufficient power to raise the forearm against gravity; there was feeble voluntary contraction in the extensor carpi ulnaris and in the small muscles of the ulnar half of the hand; no voluntary contraction could be made out in any other muscles of forearm or hand, and considerable wasting had taken place. Sensation was returning on the radial side of the forearm; on the rest of the forearm it had become approximately normal; on the ulnar half of the hand painful stimuli and light touch were appreciated, and there was some hyperalgesia; the rest of the hand was anæsthetic and there was a trophic sore on the thumb.

After another six months, considerable recovery had taken place in the extensors and flexors of the wrist and fingers, and in most of the small muscles of the hand; the patient could lift light objects with the left hand and could hold a fork and make some use of it. Pin-prick was appreciated down to the middle of the fingers in the median distribution, light touch was felt down to the wrist and all over the ulnar area. There were trophic sores on the index and middle fingers.

Two years after the accident, motor recovery was good except that there was no power of flexion of the index finger. Sensory recovery was approximately complete except over the last two phalanges of the index finger and the last phalanx of the middle finger; over these not even pressure was appreciated.

During the last six months some return of sensation has occurred in the terminal phalanx of the middle finger.

Old Injury of the Brachial Plexus.—ANTHONY FEILING, M.D.

V. B., male, aged 60, involved in motor accident on September 19, 1928; was thrown off his bicycle but does not know exactly how he was injured. Neck of left humerus was fractured, and arm was placed in plaster for ten weeks. There was also extensive bruising of neck and shoulders after the accident. When plaster was removed left arm was found to be quite powerless.

Condition on admission to hospital, January, 1929: Complete paralysis of all movements at shoulder and elbow; very feeble power of flexion and extension of wrist and fingers; atrophy of muscles of shoulder-girdle and of whole upper arm; slight œdema of forearm and hand; loss of biceps-, triceps-, and supinator-jerks; some diminution of sensation to pin-prick over distribution of the sixth, seventh and eighth cervical root areas.

Electrical reactions: Muscles of left upper arm, no reaction to faradism or galvanism; forearm; response to faradism in extensors of fingers and in extensor carpi ulnaris; no response to faradism in extensor carpi radialis or supinator longus; flexors of wrist and fingers responded feebly to faradism.

Patient was treated assiduously by massage and electro-therapy for six months with very little benefit. Now, about eighteen months after the injury, there has been slight improvement in power of the hand and wrist but none in the movements of the elbow or shoulder.

Section of Urology.

[*May* 22, 1930.

DISCUSSION ON THE TECHNIQUE AND RESULTS OF PROSTATECTOMY.

Mr. Kenneth Walker: I have always been impressed by the discrepancies that exist in the technique of prostatectomy as practised by the English school of urology. Now the nearer we are to perfection in technique, the fewer are the variations that we find, and the fact that we are so widely different in our technique in this operation surely shows that we are very far from having attained perfection. Therefore I think that this discussion, if it is carried out in the right way, should produce some valuable information. And when I say "carried out in the right way" I mean that since we learn far more from our mistakes and our difficulties than from our successes, I think we shall gain more if we lay stress on our failures rather than on our victories.

We will agree, I think, that in prostatectomy there are three major difficulties with which we are likely to meet during convalescence: hæmorrhage, sepsis, and, possibly, post-operative obstruction. It is to the complication of hæmorrhage that I shall devote the greater part of the time at my disposal.

I think it is better to approach this subject in a historical spirit. Like everybody else, I began with the old Freyer operation, but I spent so many unpleasant hours after the operation, waiting for the telephone to tell me that there was more oozing than there should be that, as soon as possible, I determined to adopt some measure that would ensure the arrest of hæmorrhage. And the first attempt, as with most other people, was with the use of packing. Let me confess that with every method of dealing with hæmorrhage I have had my failures. I have lost a case through faulty packing. There are, moreover, disadvantages of packing even when it is effective, particularly the encouragement of sepsis, and the difficulty and pain occasioned by its removal unless an anæsthetic be employed. So when hæmostatic bags were introduced I welcomed and continued to employ them for at least two years. Then I had several cases in which a bag did not satisfactorily control the hæmorrhage. I think a bag, by exerting pressure on the vesico-prostatic ledge, can prevent hæmorrhage, but it is impossible, in many cases, to get a bag to exercise uniform pressure on the prostatic cavity, and my unsuccessful cases were undoubtedly the result of faulty fitting.

Some years ago Sir John Thomson-Walker read his paper on the open method of prostatectomy. This was undoubtedly a move in the right direction, for it was an attempt to bring a primitive procedure into line with other operations and to substitute for blind enucleation, precision and visual control. I took up the open operation, but though it was excellent in many cases, there were cases for which it was manifestly unsuited, at any rate, in my hands, mainly on account of the extra time required for carrying it out. Moreover, there were stout patients, and patients in whom the prostate was very low in the pelvis, and the prostatic bed very difficult to expose, for whom I finally gave up this operation.

Still later we have seen an attempt by Harris to improve still further the operation by plastic surgery on the prostatic bed. All I can say of this is that I have not yet had sufficient experience of it to be able to pass final judgment on Harris's technique, but I know that with a sharp boomerang needle one can cause considerable hæmorrhage during the process of putting into position the lateral sutures required for obliteration of the cavity.

The result of my experiences has been that I am convinced that one must vary one's technique with one's patient. If I can do so, I perform the open operation, but I do

not allow myself to be tempted into spending a long time in trying to secure bleeding points deep in the prostatic cavity. If I cannot control hæmorrhage quickly and completely after placing my sutures, I lightly pack. Lately, in cases not suited for the open operation I have gone back to packing, because I find that with a Marion's tube and frequent instillations of mercurochrome I can prevent any severe sepsis. But how long to leave in the packing is a difficult question. Hæmorrhage may occur two, three, four or five days after the removal of the packing. So, like Marion, I am extending the time for which I leave it in. The chief disadvantage of this is that it undoubtedly increases the amount of painful spasm from which the patient suffers.

Coming to the question of sepsis, I think the only point I have time to mention here is the part played by a urethral catheter in post-operative technique. I do not, as some surgeons do, tie in a catheter at the time of the operation, for I find it more useful at a later stage of convalescence, in draining the rather septic bed of the prostate, and, still later, in promoting the final healing of the wound.

There is one minor complication which I appear to have experienced too frequently, and that is adherence of the bladder mucous membrane to the scar, so that secondary suture has been required. I shall welcome any advice on this subject. I am not conscious of having failed in these cases to drop the bladder down before suturing the abdomen.

Mr. Swift Joly: On looking up the records at St. Peter's Hospital I find that I have performed 576 prostatectomies there, with fifty deaths (8·7%). This series includes both complicated and uncomplicated cases. Apart from sepsis and renal back-pressure, the most common complications were stone, new growth and diverticulum. I have also included a few cases in which an unsuspected carcinoma was discovered on microscopic examination of the gland, but have excluded cases in which cancer was diagnosed either before or during operation.

This series may be divided into three main groups, according to the type of operation performed. The first consists of 198 cases operated on by Freyer's method—that is a short abdominal incision, digital enucleation of the prostate aided by a finger in the rectum, and hæmostasis by means of irrigation with hot lotion. In the later cases I made it a rule to pack the prostatic cavity whenever I thought the hæmorrhage was not controlled by irrigation. In this group there were nineteen deaths (9·5%). In spite of every precaution four of these patients died within twenty-four hours of the operation, from the combined effects of shock and hæmorrhage. The disadvantages of this operation are (1) inadequate hæmostasis, (2) shock, (3) sepsis and (4) risk of secondary contraction at the internal meatus. I feel sure that the presence of tags and partially separated fragments of the prostatic capsule had much to do with the chronic infection which followed the operation, and this, in turn, was a predisposing factor in the formation of secondary contractions.

The second group comprises 159 cases in which a modified operation was performed. The long incision advocated by Sir John Thomson-Walker was employed, the prostate was enucleated, usually without the insertion of a finger in the rectum, a wedge was cut from the posterior lip of the internal meatus, any tags that could be felt were removed, and hæmostasis was effected by a large gauze plug which was inserted partly into the prostatic cavity and partly into the bladder. The plug was made of vaselined gauze, and was soaked in acriflavine before use. There were fourteen deaths among these cases (8·8%). There was no immediate mortality, but one patient died the day after the plug had been removed. In this case, death was chiefly—if not entirely—due to recurrent hæmorrhage. This operation had several disadvantages, and I would not have used it so extensively if it had not been that the death-rate was at first very low. The principal disadvantages were (1) pain, (2) hæmorrhage after the plug was removed, and (3) sepsis. When the plug was in

place there was always a considerable amount of painful vesical and rectal tenesmus. These violent contractions of the bladder often continued after the packing was removed, and gave rise to serious hæmorrhage. In some cases I think there was more bleeding with the pack than there would have been had none been used. I usually removed the pack after forty-eight hours. This was always a painful procedure, necessitating either gas or an injection of morphia beforehand. If the pack was left in place for more than forty-eight hours the bladder usually became very dirty.

The third group is composed of 207 cases in which Thomson-Walker's operation was performed. His technique was followed exactly, so I need not recapitulate it. This operation undoubtedly gave the best results. There were seventeen deaths in this group (8·3%). There was no mortality during the first week after operation, deaths from shock and hæmorrhage being completely eliminated. It is interesting to note that shock after this operation is less than that after any other, although the patient is longest on the table.

A series of blood-pressure readings were taken during operation in a considerable number of cases, and although they varied according to the type of anæsthetic used, those for each anæsthetic agreed closely with each other. Under open ether there was no fall in blood-pressure during the abdominal incision and the opening of the bladder; there was a slight fall during enucleation of the prostate, which continued in a slighter degree while stitching up the bladder neck. As soon as hæmostasis was effected the blood-pressure began to rise again, and at the conclusion of the operation it was approximately the same as at the beginning. In no case did the maximum fall exceed 10 mm. of mercury. When a spinal anæsthetic was used the maximum fall was from 30 to 40 mm., but when an injection of ephedrine was given beforehand it did not exceed 15 to 20 mm.

I believe the Thomson-Walker operation is the best that has been introduced up to date. Not only is the shock less, but sepsis is definitely diminished. There is little or no pain after the first forty-eight hours, and there are comparatively few complications. There was one case of reactionary hæmorrhage, and one of moderately severe secondary hæmorrhage in this group. The cause of death was as follows: probable pulmonary embolus on the ninth day, one case; myocarditis, one case; influenza, one case (this patient also had a reactionary hæmorrhage); pneumonia, four cases; pyelonephritis, ten cases. There was no case of secondary obstruction at the internal meatus.

Twelve cases, all operated on in 1921, still remain to be accounted for. They have not been included in the foregoing groups, as they were operated on by different methods, which were tried experimentally. Three of them were operated on by a flap method very similar to that described by Harris. In two the flap sloughed, while in the third it grew across the internal meatus, and completely separated the bladder from the urethra. I had to reopen the bladder and remove the obstructing septum. In the remaining cases I passed a traction suture through the projecting middle lobe, incised the mucous membrane round the internal meatus, and enucleated the prostate while pulling on the suture. This method makes the enucleation extremely easy, but I gave it up, for two reasons. The first is that it is only applicable in cases where there is a definite intravesical projection of the prostate, and in such cases the enucleation is always easy. It cannot be employed in the difficult cases in which the gland is entirely extravesical. The second objection is that one tends to make the incision through the mucous membrane too far out, and remove too much of it. In one case I found that I had left a large bare area surrounding the internal meatus, and had considerable difficulty in bringing the vesical mucosa down to it.

After-treatment.—At the operation I leave a Marion's tube in the bladder, where it remains for four days. This tube has an angular extension, and drains the urine

into a receptacle placed between the patient's legs. On the fifth day it is replaced by a self-retaining suprapubic tube, which is left in for a week. On the eleventh day a catheter is passed, and tied in, while the suprapubic tube is removed. The catheter is left in the urethra until the sinus is healed. In most cases the bladder wound closes between the fourteenth and the sixteenth day. The bladder is washed out twice a day with a dilute solution of silver nitrate until the sinus is completely closed. The object of this method is to keep the wound dry during the whole of the convalescence, and in this respect it succeeds admirably.

Mr. John Everidge: *Selection of Cases.*—The clinical picture I would put first and reinforce the opinion so gained with laboratory tests, of which blood-urea estimation and the urea-concentration tests are the most important. In considering the general health of the patient I would lay special emphasis on the assessment of the myocardium, so far as this can be made. Tests of energy strains, by walking rapidly upstairs, etc., are valuable. The blood-pressure should be taken on several occasions.

Preliminary Treatment.—A few days in hospital or a nursing home is a great advantage, to get the patient used to new surroundings, to gain confidence, to accustom him to suitable diet, and to regulate the bowels. Cleansing of the mouth is desirable, and if there is ample time carious teeth should be extracted, but if time is limited it is far wiser to leave the teeth than to remove them a day or two before operation. The patient should be grouped, in case of subsequent necessity for blood-transfusion. Three days before operation I give glucose and alkalies; these are continued until the patient has passed through the danger period. The alkalies are then substituted by acid sodium phosphate and hexamine.

The Anæsthetic.—My routine is light open ether with novocain infiltration to the rectus muscles. The amount of ether given is quite insufficient to give relaxation, but ½% novocain injected on either side in two places, (1) at the level of the umbilicus, and (2) half way between that and the symphysis—well into the muscle sheath—gives all the relaxation necessary, and I find there is no greater difficulty in removing the prostate with this method than with stovaine anæsthesia. I have had considerable experience of stovaine and like it very much, but I have seen one or two cases where the shock was alarming, and for that reason I have given it up. I have used spinocaine, and it has been quite satisfactory. I have generally found that the drop in blood-pressure is less with the novocain-ether method than with a spinal anæsthesia.

The Operation.—The Thomson-Walker operation I regard as the ideal one, and if the technique is faithfully performed according to Sir John's directions, post-operative obstruction is almost impossible, and the likelihood of hæmorrhage greatly diminished. In those cases in which the patients are not qualified for this operation I perform the two-stage operation. I avoid a buttonhole incision for the first stage and make the incision about 2½ in. long, unless it is likely that the second stage would have to be deferred for longer than three weeks. When the second stage will follow after a short interval and a longish incision is accordingly made, it is important to fix the tube at the upper end of the wound. This keeps the peritoneum out of the way and gives more space to work in when re-opening the wound for removal of the gland. In this way much time is saved, and the re-opening of the wound and the removal of the gland only take two or three minutes. In the hands of the experienced the risk of leaving tags and a ledge is slight, and I do not know of a single case of two-stage prostatectomy which I have done in which there has been obstruction.

At the preliminary operation, although the parietal wound is made fairly large, the bladder is drained through a small hole with the aid of a trocar and cannula and a malacot tube. A watertight drainage can practically always be guaranteed and the patient, as a rule, is extremely comfortable. I nearly always have a general

anæsthetic given for the first stage, for although I appreciate the fact that it is not necessary and that the operation could be done perfectly well under local anæsthesia, I think that this first anæsthetizing is a very good "dress-rehearsal" for the big operation. It enables one to ascertain how the patient takes a general anæsthetic.

Prevention of Hæmorrhage.—If the Thomson-Walker operation has been carried out and suturing at the bladder neck, either interrupted or continuous, effected, post-operative hæmorrhage is to a large extent prevented; however, there are a few cases in which there is oozing from the bottom of the cavity, and in these it is best to use light gauze packing. I avoid the Pilcher bag in these cases as I think the pressure of it may disarrange the suture. Where no suturing is done as in an early second-stage operation, I have resorted to packing with iodoform gauze for a considerable time, but have now returned to the Pilcher bag, with which I am well satisfied, for the risk of hæmorrhage when removing gauze is considerable. The precision and accuracy of pressure obtained by inflation and deflation of the bag and traction on the catheter are in its favour. Deflation and relaxation of traction should be done as early as possible; prolonged bag pressure undoubtedly predisposes to sepsis.

The In-tied Catheter.—Where a Pilcher bag is not used the question of an in-tied catheter arises, and in many cases I think it has advantages, especially when sepsis is already established, for by its use either continuous or intermittent irrigation can be carried out at will. Subsequent to the removal of the catheter or bag I usually adopt the Janet method of washing out. The lowered resistance of the sphincters makes this easy, and a pressure of two feet is usually ample.

Hæmorrhage remains the bugbear of prostatic surgery. It is not only immediately dangerous, but it conduces to sepsis, from which there is the great risk of ascending infection and therefore of uræmia. Many patients, borderline cases, will do well, but hæmorrhage destroys the whole fabric and brings out any inherent weakness. One of my patients was doing well until the day hæmorrhage came on, and then he rapidly passed into a comatose state; a blood examination here showed not uræmia but hyperglycæmia, the report upon it being that it was 0·8%. Two previous examinations had failed to show sugar in the urine.

I regard those cases of mildly persistent hæmorrhage as almost the most dangerous, from one's reluctance to disturb the patient by such active interference as repacking or replacement of a Pilcher bag. One goes on from day to day hoping bleeding will stop, and tries all the expectant remedies. Suddenly the picture changes, the temperature perhaps goes up, the tongue is found to be dry, and it is obvious that uræmia is now threatening. At this stage transfusion, if not previously done, must be carried out without delay. It offers the best hope of saving life.

Post-operative Ileus.—Amongst the commonest post-operative complications is the accumulation of intestinal gas. In milder cases the patient gets nothing more than degrees of colicky pain, and if the accumulation is mainly in the rectum or pelvic colon, there are frequent bladder spasms. Occasionally distension may be acute, and actually endanger life from pressure on the diaphragm and embarrassment of the heart's action. Many years ago two of my patients died from this condition. How does this come about? There are several factors at work. It may occur, no matter what anæsthetic is used; I was never convinced that it was more frequent after spinal anæsthesia. My own feeling is that it occurs from several causes: (1) The patient is often old and feeble, has an atonic intestine, and a flabby abdominal wall, therefore he has not a good natural power of expelling flatus. (2) Anyone who has had an abdominal incision realizes the pain, during the first few days, of contracting the abdominal muscles to assist defæcation or expel flatus. (3) The patient is nursed in the Fowler position, is probably planted on an air ring, and there is very considerable resistance at the anal orifice.

Natural expulsion of flatus is therefore a difficult matter in the first few post-operative days, and so gas accumulates in the rectum, then the rest of the colon—

and ultimately, by a vicious circle, all the intestines—become blocked with gas. If, therefore, the rectal accumulation can be prevented from the time of the operation onwards, there is a probability that this may all be prevented. Accordingly, for the last five years, I have made it my routine practice, in cases where ileus is likely, to fix a small tube in the rectum on completion of the operation and retain it there for two or three days. As well as serving to allow the passage of flatus it is useful as a means of introducing bromide, glucose, stimulants, etc. On the morning of the second or third day castor oil is given by the mouth, and 4 oz. of warm olive oil run up the rectal tube; the tube is immediately removed, having by this time fulfilled its purpose. My house-surgeons, sisters and nurses are universal in its praise. I often ask them if they would like me to go on using it and they invariably answer in the affirmative. The patient rarely complains of any local discomfort. Since using it I have not had a single case of post-operative ileus which has given me the slightest anxiety.

Results.—In regard to mortality there are certain factors which will influence the figures of any one surgeon. The most important will be the nature of his practice. Those devoting themselves largely to hospital and infirmary work must necessarily get an unduly high mortality, for several reasons: (1) Hospital patients present themselves in a more advanced stage of illness. (2) Changing resident and nursing staff. To procure the best results a permanent team is a factor of the highest importance. (3) The impracticability of a catheter life, or a life of suprapubic drainage.

For these reasons an operation, which may be highly hazardous, not infrequently has to be undertaken. There is no alternative. Were there unlimited hospital beds or infirmaries or institutions where patients could be well looked after, living a catheter-life, or the life of suprapubic drainage, the mortality in this class of practice would be much lower.

Reviewing the mortality I find in a series of 150 cases of prostatectomy, eight in which death was directly or indirectly due to hæmorrhage. In many of these operation was performed at a distance, and I was not personally conducting the after-treatment. In two, continuous irrigation was being carried out immediately after the operation, and there was not precise enough recognition of the amount of blood which was being lost. In two, hæmorrhage came on after gauze plugging was removed, one patient died in the coma of hyperglycæmia (already referred to) and the other from uræmia. In four cases hæmorrhage came on late, about the sixteenth day, when the wounds were healed, or all but healed, and the patients were passing water naturally. In each of these cases the patients died from uræmia.

In four cases death took place from primary shock. In three of these stovaine anæsthesia had been employed, and in one the patient had had a local injection of novocain and light ether inhalation.

Of the remaining fatal cases, one patient died from pneumonia, and one from pulmonary embolus on the fourth day. One died from pulmonary embolus on the twenty-first day; the wound had long since healed and he was passing water naturally, and was actually walking out of his room on the way home. One died from uræmia on the twenty-fourth day. This was probably the result of an ascending infection and pyelonephritis.

SUMMARY.

150 cases with 16 deaths	10·66%
Of the 150 cases—	
One-stage operation was performed in 93 cases	62·0%
Two-stage operation was performed in 57 cases	38·0%
Deaths:	
One-stage—	
Nine deaths in 93 cases	9·67%
Two-stage—	
Seven deaths in 57 cases	12·28%

In glancing at these figures it might be assumed from its higher mortality-rate that a two-stage operation was the more dangerous, but, of course, this does not follow, because a two-stage operation is obviously performed in cases presenting a less hopeful operation outlook.

Mr. A. Clifford Morson: I am a supporter of the open method of suprapubic prostatectomy, not only for adenomatosis, which is the common cause of simple enlargement, but also for carcinoma.

From addresses given by distinguished foreigners on the advantages of the perineal route, I am convinced that Dr. Keyes, of New York, is right when he describes the advocacy of this method as " a passion."

So long as permanent incontinence is a possible complication I cannot believe that any surgeon will endanger his reputation by performing this operation in private practice.

The technique which I employ is similar in every respect to that introduced by Thomson-Walker, except that I think the hæmostatic stitch is unnecessary if the adenomatous mass is enucleated with a minimum of damage to surrounding parts, and if the individual vessels are picked up and ligatured.

Nor do I like the tied-in catheter at the time of operation. It appears to me that it adds to the patient's discomfort, does not drain well, and serves no useful purpose, provided the posterior flap of mucous membrane has been successfully divided.

As a pupil of the late Sir Peter Freyer, I know from personal observation and experience that the immediate results of his technique have given a mortality-rate so low that as yet they have never been surpassed, but post-prostatectomy obstruction occurred much too frequently. This disastrous complication has been eliminated by the Thomson-Walker technique.

A criticism has been raised that there is more shock with the open method. I agree that this is so in the hands of clumsy and slow surgeons. I consider that rapidity of operation, consistent with thoroughness, is all-important. Further, the use of the scalpel as a saw instead of as a knife, and the unnecessary clamping of small blood-vessels in the abdominal wall, produce more shock than anything else. It is a fallacy to suppose that hæmorrhage causes shock. Fumbling in enucleation and rough handling of tissues explain many a death in prostatic surgery.

It is generally agreed that the post-operative convalescence of a prostate case never runs an absolutely aseptic course, but I maintain that the open method and the after-treatment in vogue at St. Peter's Hospital have eliminated sepsis as a cause of death. What has not been eliminated is the danger of infection of the seminal vesicles. This is brought about by the fracture of the ejaculatory ducts at the time of enucleation and the escape of bacteria containing fluid from the bladder base down the ducts to the seminal vesicles. Within a short space of time these little ducts are sealed up and the seminal vesicles then present perfect culture tubes. The manifestation of the infection may be immediate, or remote, more often the latter, which explains why some surgeons maintain that they never see orchitis in their practices.

Months may elapse after prostatectomy before the latent sepsis flares up and spreads along the vasa deferentia to the testicles. One means of preventing this complication is the excision of the seminal vesicles at the time of the enucleation of the prostate, as suggested by Thomson-Walker, but, as he himself points out, is hardly justified, on account of the increase of severity of the operation.

In 1923, at the General Meeting of the British Medical Association held at Portsmouth, I read a short paper on the division of the vas deferens in prostatectomy for the prevention of orchitis. This paper was published in the

British Medical Journal, 1923, but few surgeons in Great Britain have felt disposed to adopt this technique. It is now practised by all urologists of repute on the Continent and by a large number in America.

The technique is simple, and in less than five minutes both vasa can be isolated and divided. Before the operation on the bladder is commenced, the vas is felt for among the other constituents of the spermatic cord through the skin of the upper part of the scrotum. It is then held firmly between the finger and thumb immediately beneath the skin. A small incision a quarter of an inch long having been made, the vas deferens can be picked up with a pair of toothed forceps and pulled out of the wound. The loop of duct thus exposed is freed from the loose cellular tissue which surrounds it, and excised. Contrary to my original practice, no ligatures are used.

I have carried out the technique just described on every case of prostatectomy both in hospital and private practice, for the past nine years, and I guarantee to my patients that whatever other complications they may have, they will never suffer from orchitis.

Cases have been recorded in which, following division of the vas, a swelling of the testicle has occurred. This is due to error of technique and observation. If the tunica vaginalis is punctured, a hydrocele may form, a prick of a vein will produce a hæmatoma, and sepsis will cause cellulitis around the testicle, suggestive of orchitis.

Should vesiculitis follow prostatectomy and division of the vas deferens, the infection will spread as far as the point of dissolution of continuity of the duct, and a tender swelling can be felt at the external abdominal ring. A small abscess may even form here. The result of this minor procedure proves that infection from the region of the prostate spreads to the testicle along the vas and not by way of the blood-stream. It also demonstrates that the lymphatics concerned as carriers of infection must be in the wall of the duct, for the latter only is divided.

If a patient is suffering from quiescent orchitis at the time of prostatectomy, it is obvious that the infection within the organ will be unaffected by the division of the vas.

As sterility is caused by the rupture of the ejaculatory ducts during the prostatic enucleation, division of the sperm-carrying tube in any other part of its course is of no consequence. When old gentlemen, after prostatectomy, flatter themselves that they have become fathers, it is wise to leave the truth and the other man to take care of themselves.

Finally, I propose to give you figures of the results of the last 128 consecutive cases of prostatectomy performed by myself at Peter's Hospital. Cases in private practice are excluded, so as to avoid any suggestion of attempt to improve on purely hospital statistics. The results in private practice are notoriously better than those in hospital practice, but it is difficult to obtain an unbiased view of statistics from the former.

I am indebted to Mr. Edwards, the resident surgical officer, for the compilation of the table.

Of 128 cases, 26 were complicated by such pre-operative conditions as stone, stricture, diverticulum, etc. The remaining 102 included cases of pre-operative retention and uræmia.

In two of the 26 cases complicated by stone, etc., the patients died, making a mortality-rate of 7·69%. Among the remainder, namely, 102, seven patients died, giving a mortality-rate of 6·8%. The total mortality-rate therefore for 128 cases is 7·03%.

The average number of days these 128 remained in hospital after prostatectomy was 28·5.

Sir John Thomson-Walker: The operation I advocate consists in making a long incision, and enucleation with the finger. The patient is then placed in the Trendelenburg posture, the base of the bladder is fully exposed by retractors, nodules and tags are removed, and the opening from the bladder into the prostatic cavity is enlarged, the hæmorrhage is then controlled either by continuous suture, or by a suture round each bleeding vessel. If bleeding still continues from the deeper part of the cavity, I do not prolong the operation, but pack round a catheter which I have drawn well into the bladder. A large tube is placed in the bladder, and a smaller one in the pre-vesical space, to prevent pelvic cellulitis. I always use a retained catheter in the urethra in order to facilitate irrigation. The catheter is not used for drainage at this stage. I prefer continuous irrigation, and it is very easy to carry this out with a retained catheter and a Hamilton-Irving box. This box is very important in the after-treatment. We put a bag of boracic crystals inside the box over the end of the tube, so that when the urine wells up it forms boracic lotion, and there is an antiseptic " atmosphere " round the end of the tube. I place an antiseptic dressing on the end of the penis and also on the end of the catheter, because it is difficult for nurses, and even house-surgeons, to realize that the end of the catheter is as much open to the bladder as is the end of the suprapubic tube. Constant irrigation is carried out with a very weak nitrate of silver solution for three or four days after the operation, until the urine is either only slightly smoky or quite clear. Both tubes are removed on the fifth day, and if necessary I insert a smaller tube instead of the larger one. The catheter is removed on the seventh day.

If the patient has not passed urine by the twelfth day, I put in a catheter again, using it for drainage.

This operation is designed to prevent sepsis, to control hæmorrhage, and to prevent obstruction; and, from my own experience and what previous speakers have said, it seems to have been attended with a large measure of success. I think it does go very far—not completely, but very far—to reduce those three grave complications.

As to the time of healing, for the last six years I have taken particular note, in my private cases, of this. On the average, urine was passed on the 11th day; the wound healed on the 16th day.

Can all cases be treated by this operation? I say they cannot; there is no operation into which all cases can be fitted; you must select the operation according to the need of the patient. This is the routine operation which I perform, and the number of cases in which I do not perform it is not very large. In cases of severe sepsis of the bladder and infection of kidneys one must drain the bladder by suprapubic cystotomy. If I have a case of chronic retention and renal failure, and after draining with a catheter for fourteen days, or three weeks, the kidneys are still in a bad condition, I drain the bladder by suprapubic cystotomy and let the patient go away for an indefinite time. In those cases prostatectomy becomes a secondary prostatectomy. I think the open operation is unsuitable for the majority of cases of secondary prostatectomy—it is too severe. The scar tissue in the abdominal wall must be removed and the bladder mobilized. The bladder is contracted and the base is difficult to expose. There are, however, a few secondary cases in which there is little scar tissue and the bladder is distensible, and the open operation can be carried out satisfactorily.

There is another type of case in which it is wiser not to spend too much time on any operation, and that is the case of the very old and feeble patient, or the patient who has serious complications, such as bronchitis. In these cases, I get through the operation as quickly as possible, and I think that for these patients the Trendelenburg position may be too severe, so if I have to perform prostatectomy I do it by the closed method.

The open operation is not a long one when the operator is accustomed to it and team work is practised. It takes about seventeen and a half or eighteen minutes and without the suturing of the bladder base, it can be done in thirteen minutes. Mr. Joly said there was less shock than after the Freyer method and I agree with him that shock is not really a necessary factor in an open operation. Time after time I have pointed out that the patient is really as well as when we began the operation.

I have looked through the results of suprapubic prostatectomy. I took up my private cases for eleven years (1919-1929) so as to compare them with hospital cases. During these eleven years I have operated on 472 cases of simple enlargement of the prostate, and the mortality was 5·2%. In one year (1921) the death-rate was down to 1·7%. In 1923 I had operated on a series of cases with a mortality of 2·1%. Then in 1920 my mortality was 11·4%, and in 1927 it was 9·7%. So one may get a series of cases which completely upsets one's previous statistics. And I went through all the statistics at St. Peter's Hospital for 29 years. There were 2,691 cases, operated on partly by the Freyer, partly by the open, method, and the mortality was 9·9%, an extremely good figure, considering that a number of surgeons had contributed to the result. The general hospital cases were collected from all over the country; there were 3,451 cases done in general hospitals, with a mortality of 19·5%. I have tried to ascertain why one gets better results in private than in hospital, and I think it is, largely, because in private the cases, after operation, are attended in separate rooms, by specially trained nurses and by the surgeon who has performed the operation and who has had a long training in the complications and the rapid treatment. Again, the difference between the results at St. Peter's Hospital and those at general hospitals is evident. I think it is due to the fact that the nurses at St. Peter's are nursing the same type of cases all the time, and the hospital is given up largely to that type of case; there are small wards, which are kept much warmer and have fewer draughts than is the case in the wards of large general hospitals Moreover, the house surgeons at St. Peter's hold the junior post for six months before they are allowed full responsibility.

The question of packing has been raised. It is not necessary to keep packing in longer than 48 hours. If when it is taken out there is bleeding, it may be replaced. But after 48 hours, I think it a mistake to leave it in, because there is a tendency to sepsis. Iodoform gauze is the best material with which to pack.

I agree as to the danger of ileus as a complication, but I think it is a toxæmia, and largely due to septic teeth or to a septic condition in the intestine before operation; there is a paralysis due to interference with the sympathetic nerve supply, rather than a direct effect.

Mr. G. E. Neligan: For my prostate cases in hospital I have a special card, and I have taken the last 100 of these cards and worked out the various complications which have occurred, all the cases having been done by the same technique, both as to operation and after-treatment. If a patient's renal efficiency is not good, I put a suprapubic catheter, under local or general anæsthesia, into the bladder, and when the kidneys have become efficient I proceed with the prostatectomy as in a one-stage operation. Then I fill up the bladder and divide the vas on both sides and ligature it. I make a small suprapubic incision, stitch the bladder to the muscle—largely for the convenience of dealing with the tubes afterwards—then enucleate the prostate, and put in a Pilcher bag, which I fill up with air, and tether to the right thigh. A tube is then inserted beside the Pilcher bag into the bladder. I put on an Irving's box. At the end of twenty-four hours the tube is removed, the Pilcher bag is also removed, and a de Pezzer catheter is placed in the bladder, and dry dressings are put on with a many-tailed binder. Often the dressings have to be changed in the first twenty-four hours. I leave the de Pezzer catheter in for ten days, and I encourage

the patient to kick about in bed. I do not carry out irrigations. After the de Pezzer catheter has been removed the Irving's box is put on and remains on until healing takes place. I leave the catheter in for ten days because the chief point in the after-treatment is not to allow healing too soon. If a piece of skin, the size of a half-crown, is torn away from the arm and an antiseptic dressing is applied, the ulcer will take a fortnight or more to heal over. I look upon the cavity of the prostate as very similar, except that it cannot be kept sterile. So that healing will take three weeks, within which period the majority of complications occur. I do not encourage the patient to pass urine under three weeks. If he does not pass it then, I introduce a bougie and tie in a catheter until the wound has healed up.

In those 100 cases the mortality was 5%; one patient died from a pulmonary embolus, and one died from septicæmia and ascending pyelonephritis. The blood-urea in this latter case was 33. I performed a one-stage operation, and the patient had acute inflammation of the prostate. A third died from lobar pneumonia, a fourth from coronary atheroma. The fifth was a doctor, who announced before the operation that he was going to die, and he did so on the third day. He did not bleed, he just died, supposedly from shock. The average healing time was 25·5 days, but five took from forty to forty-four days, and two very septic cases took sixty days; in these last two strictures developed, and I had to reopen. In sixteen out of the 100 I used bougies and tied in catheters on the twenty-first day, as the wounds had not healed up, nor had the patients begun to pass urine.

Of the complications, secondary hæmorrhage took place in two cases, in each case when the patient began passing urine before three weeks. The hæmorrhage was controlled by a Pilcher bag, and one patient had incontinence afterwards. In three cases there was inflammation of the vas, but it did not go so far as the testicle, as I cut the cords. Four patients had pulmonary embolus, which in one case was fatal, and in four cases the adenoma was found, on section, to be carcinomatous. In the two cases in which there were strictures I was obliged to re-open and make a new tract.

I have looked up my previous 100 cases, in which I employed a different technique; the mortality there was 8%, and there were more complications, which I am sure were due to allowing patients to pass their urine too soon.

Mr. H. P. Winsbury White: It is gratifying to find so much unanimity with regard to the open method of suprapubic prostatectomy. It is certainly important that different methods should be fully discussed, but it is of greater importance to find so much concerted opinion concerning one of these methods, especially as a certain amount of bewilderment results from the advocacy of a number of different procedures.

Sir John Thomson-Walker has told us that he does the greater proportion of his cases in a one-stage operation by the open method, but that, in a proportion, this is not the best operation. As a surgeon's experience increases, he tends to operate on more cases in one stage, and on less in two stages, but until he has performed a large number of operations he will be wise to make considerable use of the two-stage procedure. This is a rule which has served me well. A point which is worth emphasizing about two-stage prostatectomy is the progressive improvement in health in proportion to the length of time that the cystotomy is continued, more especially if the patient is sent home and made to go about between the two operations. Even those cases which are so bad that they barely survive the cystotomy may make a remarkable recovery in this way.

I have ventured into the perineal region on a number of occasions in performing prostatectomy, not that I have any fixed opinion that this route is a better one, but knowing that the operation has been practised in America with great success, I felt

that I should like to try it. My experience is that if the proper technique is followed one can obtain a perfect result. I have performed it in twenty-four cases, and have had only one case with post-operative urinary incontinence, but I feel sure that the avoidance of this complication is a matter of careful technique. The perineal method has the advantage claimed for the open suprapubic route, that the hæmorrhage can be controlled under direct observation. There is indeed an additional one, namely, that the empty prostatic cavity can quite simply be completely obliterated.

I am perfectly satisfied from my own results that the operation is a good one.

Mr. Morson has said that he divides the vas deferens as a prevention of epididymitis. I have followed this plan myself and I am satisfied with it. I have never known epididymitis to occur after this procedure. It is a great comfort to know that one can be certain of avoiding such an unpleasant complication.

Mr. Jocelyn Swan: So far we have had considerable unanimity of ideas, but there are several points which I would like to raise, as I have found them of considerable use to me in the conduct of an operation for prostatectomy.

First, we have learned the very great advantage of an adequate incision in the abdominal wall and also in the bladder, a step first advocated by Sir John Thomson-Walker in his open operation, in that the operator can see what he is doing and no longer attack the prostate blindly, as in the Freyer operation. Another point which I have found most useful before the enucleation of the prostate is begun, is to anchor it by means of a pair of toothed forceps. I put these forceps on to the most prominent portion of the gland and by means of them bring it upwards towards the pubes. I then make a semicircular incision around the posterior part of the prominent gland with scissors, dividing the mucous membrane and the so-called "capsule," and I have been struck with the ease with which one gets into the line of cleavage in this way. In some cases I have used the endothermy current for making this incision, with the object of arresting bleeding from the mucous membrane. With the prostate fixed and with the incision made in this manner, the enucleation of the gland by means of the finger is much simplified.

I am convinced that the two complications we have to fight against in prostatectomy are hæmorrhage and sepsis. Hæmorrhage can be largely controlled by the open operation. I do not personally use a continuous suture around the posterior two-thirds of the margin of the cavity as I think this is unnecessary, but I frequently put in one or two sutures in the margin, especially if any bleeding points are present. I leave these sutures long and use them as tractors to open up the prostatic cavity, when any pieces left at the enucleation can be easily removed. I think the greater part of the hæmorrhage after a prostatectomy comes from the walls of the cavity left after the enucleation, and for some years I used to pack the cavity around a catheter, using iodoform gauze soaked in paraffin, the packing being done by means of a special instrument which I saw employed by Legueu in Paris. I found that I gave my patients increased pain by the packing, and also that its removal after three days was painful. I therefore discontinued the packing and used Pilcher's bags, and I must say I have been much pleased with them as I think they are efficacious in controlling the oozing from the walls of the cavity. At the end of the operation I anchor the bag to the right thigh, put two tubes into the bladder, one large and the other small, and I always drain the pre-vesical space by corrugated rubber. I use the catheter through the Pilcher bag for irrigation, and, unlike Mr. Neligan, I irrigate my patients twice or three times a day, or, in the presence of sepsis, I employ continuous irrigation. I remove the small drainage tube on the second day and also deflate the bag, and on the third day I remove the larger tube from the bladder together with the deflated bag, and at the same time tie in a catheter through the

urethra which I maintain for a further week, still keeping up the irrigation by means of the catheter.

I find on an average that the suprapubic wound is healed and the patient passing urine naturally within seventeen days, and if he is not passing urine after twenty-one days I do not hesitate to re-insert the catheter for drainage.

I think it is time we gave up using the term "two-stage prostatectomy." Why not call the first operation cystostomy and the second prostatectomy? After all, the first operation is designed to overcome sepsis which may be present, or to tide the patient over his renal troubles before the prostatectomy can be done. In cases in which I consider that either sepsis or renal inadequacy precludes the performance of a prostatectomy, I am strongly in favour of draining the bladder suprapubically rather than by means of an in-dwelling catheter.

Mr. Cyril A. R. Nitch: I agree that open prostatectomy is the ideal operation, in that the operator can see what he is doing and can ligature the two vessels in the rim of the prostatic cavity. But those are the only vessels from which hæmorrhage can be arrested, the oozing from the rest of the cavity, often considerable, cannot be dealt with any better by the open than by the closed method. I do not see very many cases for which, in my opinion, a one-stage prostatectomy is suitable.

I have looked through the notes of my last 100 cases; forty-one were one-stage, and fifty-nine two-stage, operations. The total mortality was 9%. In most of the one-stage cases I performed the Thomson-Walker operation; there were three deaths, making a mortality for those cases of 7%; in the two-stage cases my mortality was 10%. This means that the two-stage operation is performed for patients who are too ill and feeble to stand the one-stage; either their renal function is poor, or they have marked urinary sepsis.

The interval between the two stages of the operation varies greatly; it depends on the recovery of the patient from his sepsis or from his renal deficiency. I find that in fifteen cases the bladder had required drainage for over a month, and in three for three, seven, and fifteen months respectively, before the patients were fit for operation. All these patients recovered. In the presence of much sepsis an autogenous vaccine is always given after the bladder is drained.

With regard to control of primary hæmorrhage, I have given up bags, plugs, and packs, having tried them all. If the hæmorrhage is profuse at the time of operation I put in a pack, but I very rarely have to do this.

In seven of the hundred cases there was secondary hæmorrhage. In two of these the operation was by the open method. Eight patients had epididymitis afterwards; two of them had had a catheter tied in. When there has been antecedent epididymitis, or when there is pronounced urinary sepsis which will not clear up with proper treatment, I divide the vas on each side at the time of the prostatectomy. In two of the hundred cases there was post-operative obstruction, both after the Freyer operation.

In the two-stage cases, thirty-two had retention (thirteen with cystitis) and seventeen had over ten ounces of residual urine (several with cystitis). This accounts for the large proportion of two-stage operations in this series, for, when preliminary drainage is necessary, I always do it suprapubically as I do not consider that an in-dwelling urethral catheter gives the same rest to the urinary system. Further, the in-dwelling catheter always sets up a urethritis, mild certainly, but still a source of infection; again it is impossible to keep the end of it surgically clean.

The three deaths after the one-stage operation were due to coronary, pulmonary, and cerebral thrombosis respectively, and the six deaths after the two-stage operation were due to uræmia, pneumonia, pontine hæmorrhage, shock, and pyelonephritis (two).

Mr. S. G. MacDonald: It is not often that serious hæmorrhage occurs during this operation, but it happens now and again, notably with large, soft prostates. After an easy, clean enucleation, the blood pours out, and the cavity must be plugged quickly; if the plug is left in for forty-eight hours there is rarely trouble afterwards. For the reactionary or secondary hæmorrhage which occurs later I have not plugged for some years. The chemical pathologist groups all my cases, and when I receive a report of the functional tests I know what blood-group the patient belongs to. If hæmorrhage worries me and there are general symptoms, with rise of pulse, I have transfusion done straightaway, and that has not failed me; it replaces the blood lost and stops hæmorrhage, and obviates the forty-eight hours' constant pain followed by sepsis.

Shock is a question of degree. To eliminate it, it is necessary to abolish all the sensory paths if possible; the auditory and visual one are as important as the tactile. These latter are abolished by general anæsthesia, and then a small spinal anæsthesia given (I use only ·25 of a 10% solution of stovaine). That gives perfect relaxation and anæsthesia as long as required, and there is no shock.

With regard to sepsis, my chief objection to tying the vas, as a routine, is that though in perhaps 30% of cases there may be epididymitis, it is not a serious complication.

The important complication as Mr. Morson has emphasized is vesiculitis, and tying the vas does not overcome this.

With reference to Mr. Joly's remark that more patients go out with aseptic urine after the high operation: surely this is a question of after-treatment. The bladder cannot be drained suprapubically without its becoming infected, and it is not until the patient begins to pass water himself again that the infection begins to clear up. Usually it takes three or four weeks before the urine is clear; but if infection continues it is due generally to vesiculitis, and it becomes necessary to treat that by massage, etc.

Sir John Thomson-Walker mentioned the question of ileus. That is very important. A fat patient with a distended abdomen is a definite intestinal risk, but in a large proportion of cases ileus is a uræmic manifestation, not a septic one, I think. The difficult cases are those in which uræmia develops without any preliminary danger signals. A patient may have normal renal tests and yet uræmia may develop after operation; I lost a patient yesterday from that cause. The autopsy showed one kidney effete and half the other functionless, and yet the tests had shown a normal result. There were many scattered hæmorrhages, including some into the mesentery.

Mr. J. B. Macalpine: Post-operative ileus has been referred to by one or two speakers. I believe the most common cause of a persistent ileus is uræmia. Two very intractable cases have occurred in my practice during the last eighteen months, each of which showed a rising blood-urea which eventually reached a point above 200, and ended fatally.

Considerable unanimity has been evident in this discussion, and yet there has been a good deal of variation in the details of technique. Practically all the members of the Section appear to have adopted the Thomson-Walker technique; I also use it wherever possible, but there is one variation which might be mentioned. When the bladder is being opened, I do not allow the urine to flow out into the suprapubic space; I use electric suction, so that that space is not contaminated.

Mr. Swan says he raises and anchors the prostate. I have done that too, and I also sometimes make an incision in the mucous membrane behind it. I think, however, that we shall all shortly be employing endothermy to make the incision round the prominence of the prostate, previous to enucleation. It should prevent much of the bleeding from the mucous edges. It can also be used for making the V-incision which Sir John Thomson-Walker describes, and which, of course, is

designed to prevent stricture. This also may be done before enucleation begins, and the needle may be so used as to incise the internal sphincter. This structure has been dilated and dislocated by the advance of the adenoma, so that it encircles the hypertrophied gland at a variable point below its greatest prominence. It constitutes a tight band, the early division of which facilitates enucleation and delivery of the adenoma.

As to hæmorrhage, we have all been through the stages of bags and packs. I regard the use of the bag as unsurgical, and, if I can, I avoid it, though sometimes I am compelled to use it. Mr. Nitch has said that frequently he did not have to pack, and that many of the hæmorrhages could be left to take care of themselves, I agree with this. It may mean the patient being kept strictly recumbent for a greater time, but no pulmonary or other complications result from the recumbency.

With regard to sepsis, I am surprised that no one mentioned the method of suction-drainage. It is quite old; I think it originated in Edinburgh. I adopt it, and I think it invaluable. The suction can be produced by a water-pump or an electric pump. In this way the urine is drawn off from the bladder, and, as a result, the urine never comes up to the suture line, and so does not contaminate the suprapubic wound. Sterile urine is very irritating, and can itself lead to much sloughing of the tissues; if the urine is infected, as it is in many cases, it is even more dangerous. If, as is possible by suction, the wound can be kept quite dry, it means a shortening of the recovery time and a diminution in the amount of sepsis.

Mr. E. W. Riches: I would like to pay tribute to the value of diathermy, particularly in controlling the primary hæmorrhage after enucleation of the prostate. It is of no use to push the electrode into the prostatic bed in a pool of blood. If the cavity is dried out after the bleeding points have been picked up with long Spencer Wells forceps, the points may be sealed off and the patients sent back to bed in a drier condition than formerly.

I have treated very few cases with diathermy, and I wonder whether others have used it, and if so, what they think of it.

I have been struck by the greater comfort of patients who have to wear an Irving box afterwards, since the introduction of Sorbo cushions round the box.

Mr. E. T. C. Milligan: I now arrest hæmorrhage in prostatectomy by the routine use, in all cases, of the Pilcher bag, after experiencing anxious times over the shortcomings of the other well-known methods. All traction apparatus, splints, weights and appliances are dispensed with as unnecessary, uncomfortable and occasionally harmful.

The bag, if inserted with the anterior margin within the prostatic cavity and the posterior margin inverting the posterior lip of the cavity, will, when distended, fit snugly and securely in position, without traction, and will exert pressure sufficient to arrest hæmorrhage before the patient leaves the operating table. A procedure such as this leaves nothing to chance, and commends itself to practitioners and house-surgeons who have charge of these cases in distant nursing homes and outlying hospitals.

The bag, I would suggest, has further benefits. It remains in the prostatic cavity undistended for three days, thus preventing the cavity from collapsing, as it usually does, together with the bladder neck, after the removal of the prostate. Moreover, the products of inflammation rigidly set the walls of the cavity in this open position effected by the bag. Thus the bag contributes to the prevention of post-prostatectomy obstruction.

It is my practice to begin the enucleation of the prostate in the prostatic urethra by rupturing the urethral mucosa on each side of the anterior mid-line, with the result that in all cases a horse-shoe shaped prostate is removed in contrast to the ring-shaped prostate usually removed when enucleation is commenced posteriorly on the bladder aspect. Besides the advantage of easier enucleation, especially in difficult cases, there remains in the intra-urethral method a vertical column of untouched mucosa lining the anterior aspect of the prostatic cavity, which must be important as a focus for the subsequent epithelization of the bare areas left after enucleation. This important area of intact mucosa does not exist when a ring-shaped prostate is removed.

Nowadays the help of a finger in the rectum is rarely required during enucleation of the prostate. The objections to its use are theoretical rather than practical, and I would strongly advise its use for guidance in dealing with the difficult fibrous prostate when no definite or easily followed line of cleavage can be found. The chances of the enucleating finger entering the rectum are not negligible, and the difficulties of dealing successfully with rectal fistulæ into the prostatic cavity, once established, I have found, in cases referred to me, to be very great.

Mr. J. Gabe: I have had the opportunity of observing many methods employed by London urologists. I think bags and packing should be avoided as much as possible. Early disturbance of the rectum, by getting the bowels open or by the introduction of a rectal tube, should also be avoided, as it conduces to hæmorrhage.

I agree that the open operation is best, but I have seen a patient who went out of hospital nineteen days afterwards return a few weeks later with obstruction.

Two or three years ago I saw diathermy employed to stop bleeding, and it was very successful; the bleeding points were touched by the endothermy current. Continuous irrigation for ten days with silver nitrate solution I have found to be very good, particularly with the tubes devised by Mr. Winsbury White.

I have been struck by the frequency of pneumonia and other chest complications in these cases, and I should like to know what is thought of the suggestion that they are due to septic emboli. I have seen incontinence more frequently follow operation for the fibrous type of prostate than that for the ordinary type.

Suction is employed in several hospitals in London.

Mr. Hope Carlton: How far is Mr. Jocelyn Swan in the habit of pushing enucleation by holding the prostate with the volsellum forceps? It is the method by which Mr. George Waugh, years ago, removed tonsils with great success. I have employed it in the case of the prostate, but it took me a considerable time. The prostate is seized with volsellum forceps and, using wide-bladed forceps, and after a few touches on its side, it comes out. I would utter a warning against employing the suction method, with the Cathcart water-pump. I have seen almost every accident imaginable take place owing to its use.

Mr. Jocelyn Swan, in reply to Mr. Hope Carlton, said that he brought the prostate up by means of the forceps, made an incision with blunt scissors, and then used the finger. He did not use the scissors or any instrument in the actual enucleation. He had been using the endothermic current for making the incision, and he intended to go on with it.

Mr. Harold Dodd: I have seen several fatal results after suprapubic cystostomy performed on patients who have had acute or chronic retention of urine. In these cases what I call acute hæmorrhagic pyelonephritis has developed; I think it

is this, because of the pyrexia and the intimate mixture of blood and urine which is passed, in spite of repeated bladder washes, until death occurs. Gradual decompression is of the greatest importance in retention before operation.

At least a week of pre-operative treatment should be given before prostatectomy; too much care cannot be bestowed on this, it makes the patient "safe for surgery." It takes the form of sedatives every night (these patients usually have considerable arrears of sleep to make up), and rectal salines and glucose three times a day which give useful extra fluids and train the patients to retain the salines after operation. Work done in acute intestinal obstruction has shown that the blood non-protein nitrogen increased to a uræmic level, and that blood-chlorides diminished markedly, but that the administration of sodium chloride quickly relieved this condition, and the patient showed a corresponding improvement. I think that a partially similar condition occurs in patients with enlarged prostates and damaged kidneys, especially after operation and when ileus is present, and that by pushing the salines before and after operation, by mouth or rectum, subcutaneously or intravenously, definite improvement will result.

I have noticed hesitation in giving salines after prostatectomy, either rectally or subcutaneously, "lest it should cause hæmorrhage," but the beneficial effect easily outweighs any questionable disadvantage.

As to sepsis, occasionally after prostatectomy and instrumentation there is a rise in temperature to 102° F. or 103° F.; in these cases I give potassium citrate, 20 gr. hourly, with gratifying results, the temperature sometimes subsiding dramatically.

It is useful to have a record of the fluid intake charted with the urine output; when bladder cases are treated in general surgical wards, the nursing staff sometimes fail to appreciate the importance of pushing fluids, but the duty of measuring stresses it, especially when they are asked to give the patient a gallon of fluid a day!

Mr. Frank Jeans (President): I have only one or two impressions to mention. One is, that the adoption of the open operation is falling into line with general surgical principles. The operator can see what he is doing. It greatly prevents immediate hæmorrhage, and so it has almost obviated the need of packing and the use of bags.

With regard to the Harris (Australian) operation, I think the prostatic bed is not the place for plastic surgery. The urethra is restored naturally and the formation of the new urethra is greatly helped by the superabundant mucosa which was over the enlarged prostate. This falls into place and meets the posterior end of the normal urethra.

As to hæmorrhage, in an ordinary operation one does not expect hæmorrhage, but if it occurs the tube should be taken out, and it will be found that the bladder-wall acts as a uterus does when the placenta has been retained. When the foreign body is removed, the musculature contracts and the hæmorrhage stops.

I agree that in the case of shock a quick operation with a sharp knife is better than infiltration, and as to sepsis, that is a matter of general surgery. A Canadian surgeon told me that in cases of advanced sepsis he did a *three*-stage operation. First he passed a large catheter into the bladder and put gauze into an incision made down to, but not opening, the bladder, the advantage being that one obtained a granulating surface which would not be infected. The second stage was the opening of the bladder, and the third stage the removal of the prostate. The bed of the prostate, however, is still a surface which might absorb sepsis, and the surgeon said that 1% of the patients died from sepsis of the wound.

What strikes me as almost a final conclusion in this matter is that no single operation is suitable for every case.

Mr. Kenneth Walker (in reply): Sir John Thomson-Walker at one stage of this discussion remarked on the unanimity of opinions expressed, and I confess that I became a little anxious, but since that stage we have witnessed a justification for holding this discussion. We have had bags praised, we have had them condemned; we have had operators who congratulated themselves—with every reason—on the rapid closing of their wounds; we have had others who said that this was the thing to be avoided. We have had suction apparatus recommended and condemned. Some have thought that hæmorrhage was a trifling difficulty, others that it was the principal cause, direct or indirect, of death. I think that if a Commission had to report on the evidence of this meeting as to what was the correct way of carrying out prostatectomy, their task would be a very difficult one.

This discussion has, I think, served its purpose, in showing how much we vary in opinion and practice, and if any moral is to be drawn I am sure the President has drawn it, namely, that no set operation can be laid down as the right one, and that we must cut our suit by our cloth—we must regulate our operation to our patient's needs.

Section of Surgery.

[*April* 2, 1930.]

DISCUSSION ON AMPUTATIONS AND THEIR RELATION TO THE ARTIFICIAL LIMB.

Mr. C. Max Page: The surgery of amputations, although it represents the oldest branch of the subject, remained somewhat academical in its teaching until the experience of the Great War forced its practical aspects upon the profession. Its 50,000 amputés in this country and corresponding numbers in other belligerent nations have provided a stimulus alike to the surgeon and the limb maker, and many views based on this experience traverse the pre-war teaching on the subject. A useful handbook was produced during the war by Martin Huggins, and in 1921 Mr. Muirhead Little, who for many years worked at Roehampton, wrote a monograph which represented authoritatively the conclusions which had been formed up to that time. In the intervening post-war period, experience of unsatisfactory stumps and of limb-fitting in general has further crystallized our views on certain practical points. It is with a view to defining this attitude that this discussion has been arranged. As opener I propose to indicate what I believe to be the attitude of the surgeon to his side of the problem. Those who follow me have been in closer touch with the subject, and have far greater specialized knowledge. Their experience of faulty stumps and the problem of limb fitting in general entitles them to the attention due to the expert. Their conclusions should, I think, be of material assistance to the practical surgeon.

It will make a clear presentation of the subject simpler if the discussion is restricted to the technique and treatment of amputations in which the factor of sepsis is not present or only in a subsidiary form. I propose to state the surgeon's view of the problem in outline; Mr. Verrall will deal with some points in the technique of primary amputations and the operation for painful stumps. Dr. Kelham and Captain Maxwell will then present their evidence and views from the point of view of limb fitting and stump function.

The general aims which govern the technique of any final major amputation in either limb and at any level may be stated to be:—

(1) That the stump should provide a lever of sufficient length and power for the attachment of an artificial limb suited to the level of the amputation.
(2) That it should be covered closely by healthy, well-nourished skin or scar, both of which should be mobile on the deeper structures.
(3) That no part of the stump should be tender on pressure or abnormally sensitive.

The first question that will arise in the individual case is the level of bone section which will give the most useful stump under the circumstances. In the lower extremity I think amputations may be considered under three headings: the Syme, the below-knee type, and the above-knee type.

The Syme stump is essentially an end-bearing one, and retains a considerable popularity both in practice and as an examination gambit. During the late war amputation of this type was entirely condemned in septic cases, and it is of importance to decide how far it is of value in civil work. The conclusions of those who have seen a large number of these cases is that it is an amputation to be condemned, as a large proportion fail to bear the test of time, and in any case can only be fitted with a clumsy prosthesis. For myself, I must admit that I have seen a good many Syme's stumps which have been giving good service. At the same

time I have been called upon to re-amputate quite a large number which, for no very obvious reason, were painful and of no functional value.

The below-knee amputation comes next. This amputation gives a most satisfactory stump if six or seven inches of tibia can be left. Control of the knee can be obtained with as little as four inches. The prosthesis bearing should be taken from the bony prominences below the knee. If more than seven inches of bone is left, the skin covering is improperly nourished and the stump will almost surely ulcerate or develop eczema. It is common ground that the fibula should be cut one inch above the level of the tibial section, and I think it is quite a good thing in short stumps to take the fibula out altogether, as the head of the bone often causes an awkward friction area when an artificial limb is fitted.

For above-knee amputations it would appear that section of the femur, from four to six inches above the level of the knee-joint, gives the best functional results. Amputations through the knee-joint are obviously unsatisfactory, but it is interesting to note that the Stokes-Gritti amputation is popular in America, Canada and Germany. I do not think it has been very widely tried in this country, and the artificial limb makers have not made much attempt to accommodate it. The general experience in Pensions Hospitals has been that an end-bearing thigh stump gives trouble sooner or later and that the ideal is, as stated above, a stump containing from ten to twelve inches of femur, to which an ischial bearing prosthesis is fitted. Amputations of the thigh at a higher level can often satisfactorily control an artificial limb up to a length of four inches from the great trochanter. If less bone than this is available it is better to resect the femur at its neck, leaving the head of the bone in the acetabulum. This gives a stump to which it is easier to fit the tilting table prosthesis than when two or three inches of femur have been left in.

The subject of the level of useful amputations in the upper extremity can be stated somewhat similarly to that of the lower, but I have not any strong convictions on the subject itself, and I will leave Captain Maxwell to make the position clear.

The points in technique which seem to give the best results in the conduct of any amputation may be considered in relation to flap cutting, the bone section and nerve end treatment.

In this country it is generally agreed that the end of the bone in an amputation stump should be covered by skin only. Flaps should be so cut that their nutrition and innervation are preserved as far as possible. To this end some muscle and fascia should be included in the base of the flap. It is clear that the shorter the flaps are, the better is the irnutrition, and, in general, I think the skin may be cut as in a circular or elliptic amputation, short side cuts being made as necessary. At any rate all long flaps should be suspect, and, as far as possible, avoided. The scar uniting the flaps should come on the posterior aspect of the stump in the lower extremity ; in the upper it should lie approximately central or terminal. In designing the flaps their total lengths should not much exceed the diameter of the limb at the level of bone section. Redundant loose covering of the stump, especially in fat individuals, and in the thigh, is a great handicap to the successful fitting of the prosthesis.

I notice that in America the suture of muscles, fascia and tendon over the end of the stump is still advocated. I doubt if it has any value in the stump. Muscle in any case wastes away and there is a risk of adhesions between muscles and skin if the practice is carried out. This gives a terminal scar, which is moved by muscle contractions. This condition is liable to lead to development of friction sores when a prosthesis is worn.

A good deal of attention has been devoted to the exact method of cutting through the bone, the main aim being to prevent spur formation. I think the importance of this condition has been over-emphasized, as many perfectly satisfactory stumps show

the existence of considerable spur development. Whatever its practical importance, I think that spur formation is due to a tearing up of the periosteum at the time of section, or to the extrusion of marrow containing bone-forming cells. This can be prevented by dividing the periosteum about half an inch above the proposed line of bone section and scraping it down the shaft. The saw cut is then made through a bone denuded of periosteum. It is thought that curetting out the bone marrow for a distance of 1 or 2 cm. from the cut end, after the method of Bunge [1], also minimizes the likelihood of irregular bone formation and of subsequent tenderness. I think that if the marrow cavity is plugged with bone wax it is equally effective, but I believe Mr. Verrall has some evidence on this point.

Attempts to cover the cut ends of the bone with an autogenous or pedicled graft, as in the Pirogoff, Stokes-Gritti amputation and Biers [2] [3] osteoplastic procedure, have not been popular in this country. The aim of such amputations is to produce an end-bearing stump. As stated above, this type of stump has not found favour with those fitting limbs in this country, and, undoubtedly, if the primary amputation fails of complete success a secondary operation is almost always necessary.

The treatment of the cut nerves is important. I think that the general experience is that no procedure hitherto adopted will prevent formation of an end bulb. End bulbs may be large but insensitive, and no material handicap in the use of an artificial limb. Those which subsequently give most trouble are often formed in the smaller cutaneous branches. It is certainly desirable that the stump of the nerve should not be attached to the scar, nor should it lie where it may be pressed on by the prosthesis, but I do not think that the pulling down of the larger nerves and their section very high up has been satisfactory in practice. I have often observed severe post-operative pain, which seems to be attributable to the trauma of the nerve trunk. I think the best treatment is to crush and cut all recognizable nerves an inch or so away from the level of the bone section ; this is followed by an injection of absolute alcohol into its stump. The latter procedure at any rate prevents the origination of painful impulses from the area removed. Freedom from pain following operation is important ; this is the time when the patient's nervous system is most sensitive, and such impressions are liable to initiate the development of a sensitive or jactitating stump.

In the class of amputation under consideration careful suture of the flaps will naturally be carried out. As a general rule, drainage for twenty-four or forty-eight hours is advisable. The development of a hæmatoma in the early stage is most undesirable, and will not only interfere with the nutrition of the skin, but is liable to be followed by low-grade suppuration. I notice, however, that Wilson [4], in a recent American publication, strongly advocates no drainage, a view which is also taken by Mr. Verrall.

In this brief outline the views I have expressed apply to stumps which are expected to be final in character, the amputation having been carried out in such a way or at such a time that gross suppuration was avoided. I will not refer now to the question of amputations in the presence of sepsis except to note that such stumps are more often painful or tender than in clean cases.

The full function of the stump depends upon its normal position being maintained during the period of convalescence and on the maintenance of its musculature in good condition. It is unnecessary to emphasize these two points at the present time ; the importance of avoiding post-operative joint contracture is well understood and the use of early temporary pylons is common. A certain number of painful and unsatisfactory stumps seems always to be with us, and the treatment of these is a problem which has to be faced.

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Mr. P. Jenner Verrall: I join with Mr. Max Page in his condemnation of the Syme amputation. My first acquaintance with it occurred in the East. It is an admirable operation for the native, because he has no opportunity for wearing an artificial limb. But in Europe, where artificial limb-making is so good, we have found that the Syme stumps will stand for six or seven years and then begin to go wrong: the end becomes displaced posteriorly or to one side. So I have given up the Syme amputation; though it is good theoretically, it is not so in practice.

From this discussion we are leaving out all emergency and unusual operations.

The level at which one divides the bone and the flaps one makes will be determined largely by any scars present. I have had models made in which I have marked out the type of flaps which I consider ideal. I do not like a posterior scar in a below-knee amputation.

I have always used the postero-external flap, making an anterior incision, passing across the bevelled front edge of the tibia. After a time that scar will descend about half an inch or an inch below the bevel. I have here a cast of a satisfactory below-knee amputation which shows the position which the scar subsequently assumes.

Through the femur I use an anterior flap with a short posterior flap. I do not treat the periosteum in any special way, but divide it on a level with the bone. The usual spur on the femur is postero-internal, and if one makes a short posterior flap and a short anterior flap, the situation of the subsequent scar will be across the possible site of formation of this postero-internal spur, but if a longer anterior flap is made the scar will be further back, and will be above the postero-internal spur. I think spurs are unimportant, except when they are directly underneath the scar.

With regard to drainage: for years I used drainage at Roehampton and other places, and also in my civil surgery, but in the past year I have given it up. If in any particular case I am doubtful about it, I stitch the wound with spaced interrupted sutures, especially in the case of thigh stumps.

Splinting of the knee for below-knee amputation is very important. The stumps in my cases are not usually dressed until the fourth day after operation. If a hæmatoma forms it is usually a small one, and it becomes absorbed. A large hæmatoma may occur, and then, of course, it is necessary to evacuate it. After evacuation, the wound is resutured, and the result is satisfactory. The cases in which hæmatoma are liable to occur are those in which, when the tourniquet is removed on the operating table, the bleeding is notably small. These cases are likely to give trouble from bleeding later on.

An important point in treatment is that in all the cases of amputations it is essential that the patients should remain in bed three weeks.

With regard to nerve-ends, I treat them in the same way as Mr. Max Page does. I do not pull them down much, and still less do I avulse them. We have heard of cases of avulsion of nerve which have ended tragically. I quite agree that it is important to have a minimum of trauma. The situation of the nerve-bulb which will subsequently form must be considered in conjunction with the limb-fitting surgeon.

A hip amputation means an amputation which is situate from a point just below the small trochanter to the joint itself, depending on how much tissue there is in the buttock. In the case of a person who is a mere skeleton one can amputate through the neck of the femur. For a stout person it is well to cut lower, in order to leave a skeleton for the stump. If an anterior incision be made, dividing the femoral artery in the front and using purely gluteal flaps, the best situation for the sciatic nerve-bulb is an unshortened one, because if it is shortened the patient will sit on the nerve-bulb. If it is left at the end of the flap the patient will be sitting on the trunk of the nerve. Alternatively it can be cut off at the sciatic notch.

I condemn the use of pylons for below-knee amputations. In favour of the use of pylons in these cases it is said that the patient has early orientation in walking, and that the fitting of a pylon will save money, because less fitting is afterwards required. But against that is the fact that a patient walking on a plaster pylon has an unnatural gait, and sores are very liable to form.

For above-knee amputations the pylon is valuable, because it causes shrinking of the stump by economical means to the size needed for the fitting of an artificial limb.

One of the causes of an unsatisfactory stump is to be found in the condition of the circulation. This difficulty is generally due to the stumps being too long, as we have already been told by Mr. Max Page.

With regard to nerve bulbs, I agree with Mr. Max Page that the ones most likely to be troublesome after amputations carried out by a competent surgeon are the cutaneous ones. And the worst are those of the communicans fibularis coming down the leg, and the internal saphenous in the thigh, nerves which are not thought of during the operation because they do not usually give much trouble afterwards. The trouble is either very little or a good deal.

Next a word as to bursæ forming on the stump. My advice is that they should be left alone. If the bursa is cut out a scar is produced and is likely to be rubbed and to cause a sore. You can only send such a case back to the limb-fitting surgeon and tell him to do better next time.

With regard to sebaceous adenomata: If these must be dealt with surgically, an extensive resection of the skin should be carried out and the whole area removed. I find, however, that the use of white precipitate ointment will obviate a good deal of surgery.

Adherent scars on the end of bones constitute a troublesome condition, and an endeavour should be made to avoid it. They should not be put over spurs. If the scars are adherent, massage will free them; and gentle attention with a tenotome to free the scar from the underlying tissues may help the masseur.

With regard to unsatisfactory arm stumps, I can only speak from my own experience and that of my colleagues at Queen Mary's Hospital, Roehampton. I advise against tinkering with the nerve-bulbs afterwards. They should not be removed indiscriminately but only when the patient complains that they are tender. I advise emphatically against removing the plexus, and, still more so, against interference with posterior nerve roots.

With regard to the upper limb, we have to bear in mind that this is a sense organ to a certain extent, and the cause of most of the pain in arm stumps is of central origin. The cortex of the brain can be compared with a racing motor engine. When the sensation is cut off, the cells of the brain are hard up for something to do. It is therefore important to get the patient fitted with an artificial limb as soon as possible, and give him something definite to do.

As to "shivering stumps" in the upper extremity, some say it is advisable to disarticulate at the shoulder-joint, but our experience is against that procedure.

Having dealt with the median and ulnar nerves in the forearm we sometimes find tender ends in the radius and ulna. It is good practice in dealing with these to strip back the periosteum, dig out the medulla, and smooth the ends of the bones, making them as neat as possible, having previously dealt with the nerves.

Mr. R. D. Langdale Kelham: The point I wish to put before you is the necessity of a closer coöperation between the medical man and the limb maker, and for a greater knowledge on the part of the medical man as to what constitutes a satisfactory artificial limb. The study of artificial limbs belongs to both Surgery and Mechanics, and they cannot and should not be separate. The medical man should be in a position to know what type of fitting is best suited to his patient, and whether his instructions to the manufacturer have been properly carried out so that his patient

has received the best possible prosthesis and is able to make the best use of it. The responsibility for all this in the past has been thrown entirely upon the limb-maker. The limb-maker's knowledge must of necessity be great, but when he fails it is often because of an understandable lack of scientific knowledge and thus the patient may be incorrectly fitted or supplied with an unsuitable type of limb. Many patients suffer discomfort which is quite avoidable if further co-operation between the medical man and the limb maker can be obtained. Most of the Governments of the belligerent powers in the Great War caused a number of medical men to be trained in the science of artificial limb fitting, with the result that the war amputé has benefited: his chance of returning in a large number of cases to his pre-war occupation has been rendered almost certain and artificial limbs in general have been vastly improved, owing to the close co-operation at Roehampton between Mr. Verrall, Consulting and Operating Surgeon, and those of us concerned in the fitting of the artificial prosthesis.

The knowledge gained and the improvements made are still largely confined to the benefit of the war amputé. Can the civil population not also benefit? There are many firms manufacturing artificial limbs in this country, differing from one another widely or in detail in matters of design. Most of them can manufacture any type of artificial limb required, but undoubtedly there are some firms who, through their vast experience and specialization in some particular type, are able to manufacture and fit certain types better than others. It is impossible here to designate the various differences and qualities of limbs available. I can only give a rough general idea of the various points involved.

The methods of classifying artificial limbs for amputations of the lower extremity are many and varied.

I will divide the subject into two main headings: (1) Limbs for above-knee amputations, and (2) Limbs for below-knee stumps.

(1) *Amputations above the Knee.*—For this amputation limbs may be classified under the methods by which they are controlled by the wearer, the two main groups being the (*a*) central knee control or American type, and (*b*) the type which demands the use of a special form of pelvic band.

When speaking of an ordinary above-knee limb, I refer to one normally supplied for an average stump. This is one which measures ten to twelve inches from the tip of the great trochanter, and is, from the patient's and our point of view, the ideal stump. Length alone, however, is not a very reliable guide, as will be shown later, but a stump of this length, if otherwise normal, is capable of actuating and controlling a limb with no support other than the means by which it is suspended to the body.

For this stump a limb either of the first or second type may be suitable but factors other than length will decide which should be used for a given case.

Briefly, the first, or American type, is one controlled by rawhide cords running through a roller in the knee-piece of the limb, the roller being set eccentrically. The roller cords are attached to suspenders passing over each shoulder, and, by bracing the shoulders, an eccentric pull is exerted, resulting in extension of the limb at the knee. This is the usual type, but there are several modifications of it, all of which depend to a greater or lesser degree on shoulder movement. With such the stump is not necessarily called upon to exert itself, and so it is useful in cases in which proper use of the stump cannot be made. In this type of limb the knee-joint is of the free swing kind and may be of the ball-bearing variety.

The second type, on the other hand, has a knee-joint which is not, in the same sense, free, being of the friction variety. The stump's muscular power is necessary to actuate the limb. The precise amount of friction, and therefore of muscular power on the stump's part, can be adjusted in accordance with the needs of the case. This type of knee-action is designed to be used with the special pelvic band of the

double swivel variety. The band is attached to the socket of the limb in such a way that flexion, extension, adduction and abduction of the stump are permitted, and controlled, without the stump's position in the socket being altered. In addition, rotation of the pelvis as a whole is permissible. The function of this band is to suspend, control and support, this order being given intentionally, to indicate the value of its particular functions. With this band a single brace over one shoulder is all that is required and frequently this can be dispensed with.

The advantages, therefore, are (1) freedom from shoulder suspension, (2) nothing to restrict the chest or shoulder movements, control of the limb, coupled with a definite tendency to production of increased muscular development of the stump. The indication for the prescribing of one or other of these two types is apparent from this and other factors. The main principle of the fitting of both types is that the weight of the patient should be borne upon the tuber ischii, the socket being so shaped and fitted as to produce this result.

In either type the limb consists of a socket, a knee portion, with its contained mechanism, a shin portion and a foot, and in the second type a special pelvic band is added. These limbs—with the exception of those of the second type, which are all of metal—may be constructed of wood throughout, or of metal with a wooden socket, and possibly knee-piece, or a certalmid socket, or metal socket, the type to be prescribed for a given case depending upon the surgical nature of the case and the patient's financial status.

There are other types of above-knee limbs, but I will mention only one, which is a wooden limb having the knee- and ankle-joints constructed on the "tenon and mortise" principle, and known as the "Anglesey limb," so named after a Marquis of Anglesey for whom it was first made. This limb, though light and a fine example of the limb-maker's art, is not suitable for general use, being insufficiently strong for those who do heavy work, or are embarrassed by the cost of frequent repair bills, but it still retains a place amongst the limbs of the present day despite its antiquity.

Passing on from the ideal stump, we must now consider the type of limb supplied for stumps which do not fall within that category.

To commence with I would take the stump which, irrespective of length, is for some surgical or possibly mental reason on the part of the patient, unable to control either of the types mentioned without some additional support of a mechanical nature, that is to say, support given in order to ensure a firmer attachment between the stump and the trunk. This support may be given by one of the various types of pelvic band, from which just the amount of support best suited to the patient can be selected. Here I must emphasize that it is better not to over-support the patient, for in that case the patient will not endeavour to develop his own muscular powers as he can do when he gains confidence, in cases where development is possible.

The double swivel band has been referred to as giving support in a third degree. Next, we have the rigid band connected to the socket by means of male and female steels giving movement of flexion and extension only. This type gives the greatest support, and indications for its use should be clear.

The next type is one in which a lateral abduction joint is inserted in the male steel, and this, as its name implies, permits of a certain amount of lateral movement in addition to flexion and extension.

Next we have the joint, the male steel of which consists of a spiral coiled spring, giving still greater range of movement; the greater the range of movement the less the support.

In one type a ball-and-socket joint at the top of the male steel provides rotation and lateral abduction. Another type is represented by what is called a "security belt," containing no metal parts; this is a recent invention and now

on trial. No rule can be laid down as to which type is best; the surgeon must be guided by the individual characteristics of the case.

The length of the stump is of little importance, except that when dealing with very short stumps the indications are obvious.

The deciding factors are the nature of the stump and the mentality of the patient, muscularity, freedom from redundant tissue, absence of interference with the normal movements of the hip, the presence of painful and irremovable neuromata or scars.

A realization of exactly what the stump is called upon to do may help in the decision when the nature of a particular stump is known, as it can be best known only to the operating surgeon.

With the patient standing at attention, the stump must be braced back to the back of the socket to prevent the knee shooting or giving way, the stump must also be braced to prevent adduction or abduction of the limb. In taking a pace, the stump must lift the limb and flex the knee at the same time, retaining the limb under the patient. When the artificial foot touches the ground the stump must again brace back to the back of the socket and retain the vertical position of the limb. These essential functions demand muscular power and, in the early days of limb-wearing, concentration on the part of the patient.

The mentality of the patient should not be neglected, since it sometimes decides the type of band required.

Though length of stump is by no means all important as already mentioned, it can be stated as a matter of interest that in some thousands of cases the average length of stump fitted with a full rigid band at Roehampton is seven inches. The average stump fitted with the double swivel band is ten inches and three-quarters, and the shortest fitted with this type of band is six inches. This may appear paradoxical since it has been stated that a stump of from ten to twelve inches requires no support at all, but the double swivel band in such ideal stumps is given for reasons of suspension and control mainly, or to free the shoulders from braces or on account of some chest condition necessitating freedom of the chest.

We must next consider the case of stumps which, for various reasons, cannot control or actuate an above-knee limb at all even with support, which brings us to a consideration of the "tilting table leg." This leg was primarily designed for patients who have suffered excision at the hip-joint, and it must also be used for amputations through the neck of the femur, at the level of the lesser trochanter, or an inch or so below that level. The difficulty with regard to prescribing this limb begins when dealing with stumps which do not fall within the above categories. It is the heaviest, most cumbersome and most tiring limb of all. The surgeon should do all he can to avoid condemning a patient to wear a limb walking with which necessitates swinging it from the pelvis. Except for the socket and upper portion of the thigh, this limb is mechanically similar in design and controls to other above-knee limbs, and is made of metal or wood, though wood should never be used now if it can be avoided. The socket may be of metal, leather or certalmid, and is made from a cast of the stump. When there is any movable stump, the cast is taken with the stump at 90° flexion. If a stump is long, the bulk of the socket will therefore be appreciated.

In border-line cases the selection of the limb is most important, as a faulty selection may not only cause harm to the patient's stump and future prospects of walking, but may also embarrass him financially. Cases of this type should be dealt with from the point of view of excluding all possibility of a successful fitting with an ordinary above-knee limb. Firstly, what is the available length of stump —which for this purpose should be measured from the perineum? A stump of good length is of little use if it is of an adipose nature, whereas a shorter stump poorly

covered and conical presents better prospects of an above-knee fitting. Secondly, there is the question of muscular power, absence of which will contra-indicate the use of the above-knee type and necessitate a "tilting table." Thirdly, movements at the hip-joint should be full and free; a very slight degree of flexion or abduction does not always preclude the above-knee fitting, but it does demand the services of a fitter of the highest ability, and co-operation on the patient's part.

Circulation.—Fortunately with short stumps we are rarely if ever troubled with difficulties in this respect, and defective circulation is a rarity.

Nerve supply must be normal if an above-knee limb is to be provided for a short stump; anæsthetic and hyperæsthetic areas prohibit anything but a "tilting table." Neuromata may cause trouble, and the policy of removing them in this or other types of cases is a debatable one.

In our experience we have found that the practice of removing indiscriminately, any or all neuromata of which the patient may complain, is by no means a sound one. In a number of cases there is no doubt whatever that the operation has set up a traumatic neuritis and the last state of the patient is worse than the first. Whenever possible, neuromata should be treated by special fitting to avoid pressure rather than by operation. In the case of short above-knee stumps, however, when the necessity for a tilting table may be under consideration, the question of removing neuromata should receive serious consideration, if by doing so an above-knee fitting is likely to be rendered possible.

In one of several cases we have had in which the patients have been troubled with neuromata in long above-knee stumps, as soon as the original neuromata were removed, new ones appeared higher up; these in turn were removed, and finally the great sciatic nerve was evulsed, leaving a flail stump over which the patient had no control. The patient could not wear an ordinary above-knee limb, nor could he wear a "tilting table" owing to the length of the stump. In such a case it would have been better had the stump been re-amputated at the level of the lesser trochanter or higher at the time of the last operation, to enable a tilting table to be worn. These cases are few and extreme, but serve to illustrate several points. Severe scarring in the neighbourhood of the tuber ischii often necessitates the supply of a tilting table in stumps otherwise normal.

Some patients are of such poor physique or in such a debilitated state that they cannot stand the strain of a tilting table articulated leg in the early days of their limb wearing. For these an all-metal tilting table peg leg can frequently be supplied. This is much lighter, and the patient can get about very well with it until his general condition has improved sufficiently to permit of a fully articulated leg being supplied.

From records of several hundred cases in which the patients could not wear any other type of limb than the tilting table, it is found that the average stump length is $4\frac{3}{4}$ in., and the longest stump fitted with this limb was 6 in. from the trochanter.

Passing on from a consideration of short above-knee stumps there are several points regarding the long stumps to which I must call attention.

There was a tendency at one time which still persists in some quarters, to conserve as much as possible of the limb when amputating, and to remove only so much as is strictly necessary for purely surgical reasons at the time of the operation. By sacrificing a few inches of bone at the time, one can give the patient a trouble-free stump and, subsequently, a limb with which he can walk better and in greater comfort. There are surgical and mechanical objections to a very long above-knee stump.

A stump longer than 10 or 12 in. from the trochanter is likely to fall within that group which are more liable than any others to circulatary troubles, which after a greater or lesser number of years render re-amputation essential. The number of these long stumps is gradually but surely being reduced by re-amputation.

Mechanically, a long stump will often foul the knee-control mechanism, rendering a different and less efficient control necessary, or alternatively, it is necessary to lengthen the thigh portion of the limb and shorten the shin, producing unsightliness when sitting and upsetting the balance of the limb. Unsightliness may not, in view of some, be important, but amputés almost without exception pay great attention to the æsthetic side and object to their disability being called attention to. The upsetting of balance is, however, an important factor.

At one time amputations around the knee-joint were popular and still are in some degree, such as Stephen Smith's, Carden's, Gritti's and Stokes-Gritti's, performed possibly on the general principle of conservation of tissue but also with a view to producing an end-bearing stump. The number of cases able to take end-bearing nowadays is infinitesimal, and it is found that stumps do not do well when taking weight upon surfaces upon which they were not designed to do so by nature, at least so far as taking of the body weight upon the cut end of a bone is concerned. In limb fitting practice the weight is taken either on the tuber ischii or on the bearing surfaces of the tibia and fibula.

Stokes-Gritti's operation is still favoured in some quarters, but from our point of view it is very unsatisfactory. Apart from being susceptible to circulatory troubles as are all long stumps, the patella is rarely found in the correct position as placed at the time of operation. It is found floating at the extremity of the stump, and often seen lying on the anterior surface of the femur two or three inches above the extremity. This may be due to the patient being fitted too soon after the amputation or possibly to bad fitting. There may be other causes.

For these amputations in the neighbourhood of the knee-joint we supply what is called the "through-knee" limb, the socket of which must be of leather, blocked to a cast of the stump and laced to the thigh. The knee-joints are placed externally of necessity. The fit is usually tuber-bearing, the bulk of the weight being taken at this site. The limb is heavy and rather cumbersome and hot to wear.

Briefly, we have found from experience that, when a surgeon has a choice within reasonable limits, an amputation performed to remove the lower end of the femur never less than 4 in. above the adductor tubercle, gives the most satisfactory stump and the possibility of supplying the best type of limb.

I now come to a consideration of below-knee stumps, the ideal length of which is 7 in. from the articular line of the knee-joint.

The old site of election of 4 in., or a hand's-breadth, below the knee served its purpose well at a time before artificial limbs had reached anything approaching their present position, since it was the custom for the patient to walk on the "Chelsea peg," with his 4-in. stump flexed in a knee-cup.

The limb supplied for the ideal below-knee stump consists of a corset of soft leather, side steels with ball-bearing joints, a socket of leather or wood, the remainder of the limb being constructed of either metal or willow. If the socket is of wood, it is pulled out by hand to measurements of the stump, but if of leather it is blocked to a cast of the stump. An "Anglesey" limb is also available for this amputation, having the tenons and mortice joint at the ankle and either short or long tendons, the latter being attached to the knee-joints. In either case, shoulder suspenders or a belt are required to suspend the limb. The patient takes all his weight upon his stump, but not upon the end of it, the bearing surfaces being the anterior tubercle of the tibia, the inner head of the tibia and the head of the fibula. The corset takes no weight and is merely for support and maintenance of the position of the limb.

There are many stumps, however, which are unable to take weight in this way, alternative methods of fitting being necessary in order to relieve weight from the stump, either partially or entirely, as the condition of the stump may require. Weight can be relieved partially from the stump by means of substituting a stiff

blocked leather corset in place of the soft one; this corset will require to be longer than the usual type. By means of a full tuber-bearing corset the patient can be made to take all his weight off the stump. Needless to say either of these types renders the limb heavier, for which reason every effort is made to fit the patient with the simpler and lighter type.

Certain conditions prevent anything but a tuber-bearing limb, such as a predisposition to recurring attacks of acute bursitis, adherent and keloidal scars which become dragged upon, neuromata of a painful nature which cannot be overcome by the usual fitting. Anæsthetic or hyperæsthetic areas following some sciatic lesion render a tuber-bearing limb essential, as also does circulatory disturbance at the extremity of the stump. This latter is met with in long stumps and, when once established, usually ends in a re-amputation to the more favourable length in spite of some temporary comfort the patient may derive from the tuber-bearing corset. In above-knee limbs the metal limb is always the lighter, but in the below-knee types no advantage is gained through weight alone between the metal and the wood limb, the latter being, if anything, slightly lighter in gross weight. I am referring, of course, to the most modern type of willow limb, which in its simple form weighs, on an average, from 3 lb. 14 oz. to 4 lb. 4 oz.

When discussing above-knee stumps it was stated that there were certain stumps for which the ordinary above-knee limb was unsuitable, and which necessitated a tilting table leg; we have an analogy here, since below-knee stumps which cannot be fitted with either of the types mentioned must be given what is called the "kneeling leg."

In this type, the leather socket is made from a cast taken with the stump flexed on the knee at a right angle, producing an unsightly and cumbersome limb. Apart from the projection backwards of the lower part of the socket the limb is similar to the through-knee limb.

Practically the same surgical conditions which, in the case of the above-knee stump, necessitate the tilting table leg, in the case of the below-knee leg demand the kneeling leg and I need not recapitulate them. Only one point I would call attention to, and that is that surprisingly short below-knee stumps can and do, when well fitted, actuate and wear comfortably below-knee limbs; those which do may, however, be regarded as successful experiments. A helpful guide as to the available length of stump for controlling a below-knee limb is obtained by placing the flat of the hand against the posterior border of the stump so that the upper margin of the hand lies at the level of the knee-joint. Flex the knee fully and it will readily be seen how much stump is left or likely to be left within a socket.

I now come to Syme's amputation which is so popular with the majority and so strongly condemned by those of us who are closely connected with fitting the stump. Some of what I have stated and much of what I am about to say may, I am well aware, be regarded as heresy, but it is an honest conviction based on experience gained during the past nine years during which time I have done nothing but deal with amputation stumps and the fitting of limbs to them. The Syme stump is considered desirable because the patient can take weight on it, and is of advantage also in that he can stump about his home, if he wishes, without wearing the limb. I have little to say against either of these points so long as the stump remains in that favourable condition, from the surgical viewpoint.

But take the mechanical aspect. The Syme limb is heavier in proportion to the amount of natural limb it is replacing than any other artificial limb, relatively speaking. The amount of power exerted by this stump is so great that great strength is needed in the limb mechanism, accounting for the weight. The clearance between the stump end and the ground is so small that there is no room for a really satisfactory ankle mechanism. The bulbous extremity is such that a bulky appearance in a greater or lesser degree is unavoidable.

Returning to the surgical aspect alone: so long as the patient can take weight on the end, well and good, but if for any reason, weight cannot be taken there, the Syme, as a Syme, fails entirely, since that is its only advantage. The main point is, how long does a Syme remain good? and how long can a patient continue to take weight on the end? If the Syme stump remained sound for as long as the stumps I have described as ideal, I should have nothing against it but the mechanical objection.

Before enumerating briefly the defects to which a Syme is particularly liable and which result in re-amputation, I must emphasize the fact that when the patient can no longer take weight where he should, the various alternatives in fitting have to be tried. Firstly he must take weight as if there were a below-knee amputation, next he may have to have side steels and thigh corset added, then the corset may have to be of blocked leather, and finally may have to be full tuber-bearing, the weight of the last apparatus being greater than a limb which is supplied for a short thigh stump.

It would seem that, roughly speaking, ten years is about the average life of a Syme, as a Syme, that is to say that for ten years the patient is able to get about with or without difficulty on one or other of the fittings described, but that after that time re-amputation is performed.

The number of old Symes left is dwindling gradually as re-amputations are performed and never yet have I found a case once a Syme's, and now re-amputated and fitted with a normal below-knee limb, in which the patient has not been thankful for the change. The objection to this stump and the defects to which it is liable are very briefly these: The density of the tissues covering the os calcis and transplanted to the end of the stump should be theoretically ideal for the purposes of forming a pad or end bearing. Actually, however, their very suitability is the main cause of their failure. The pad so formed does not retain its position in many cases, but becomes forced laterally, so that the weight is taken on the side. This can be temporarily corrected by fitting, but the correction is rarely permanent. The Syme stump appears, if anything, more liable to circulatory troubles than any other long stump, the extremity becoming blue, cold and painful. It is subject to neuromata as other stumps are; these neuromata are rarely palpable, being almost pin-head in size, but they are extremely painful. Operations for these may do good but they leave scars which interfere with subsequent fitting. The amputation scar, being anterior and transverse, is liable to be dragged upon and is so greatly affected that ulceration of the scar results. Frequently the scar is keloidal and painful, piston action of the stump in the socket aggravating the condition; this alone has often prohibited end-bearing. The formation of callosities is practically certain, sooner or later, and may appear either in a transverse line beneath the anterior scar or on the bearing surface. Many of these become very troublesome and painful, and, when in the latter situation, preclude end-bearing unless operative interference is attempted, but this, though possibly curing the condition, will in itself involve a new type fitting. Cases of ulceration supervening are by no means uncommon. Periostitis of the anterior border of the tibia is a very common condition and causes painful symptoms; a method of fitting has been designed to overcome this, however, but is not, unfortunately, always utilized.

Skiagrams have been taken of a series of Syme's stumps, and though many show rarefaction of bone in the lower third of the tibia and the fibula, it is noticeable that the rarefaction is excessive in cases in which the patients complain of otherwise unaccountable symptoms.

I must mention that to overcome the objection of weight, an all-metal Syme's limb has been produced, but the results are very disappointing, the fitting difficulties being such as to render its general use impossible at the moment.

Amongst the many types of limbs of a special nature with which I have not had

time to deal I must mention two. The first is an above-knee limb known as the suction socket limb. This limb is held on partly by suction and partly by the stump's action against a series of partial or complete spiral grooves in the upper portion of the socket. In a number of cases this has proved very successful, and in such the fact that no harness or shoulder suspension is required is of great advantage to the wearer. Many, on the other hand, have not proved successful, having a deleterious effect on the stump. The limb is still on trial with a view to discovering whether it is likely to become of general utility.

The second is a below-knee limb, the new method of fitting of which renders side steels and corset, as generally understood, quite unnecessary. This enables the patient to have a limb weighing only two pounds, freedom from constriction of the thigh, and no shoulder or pelvic suspension. It is necessary, however, that the stump should be capable of taking full weight. This limb also is in the trial stage, and it is too early to express any opinion as to its general application or utility.

We now come to the final phase of limb fitting, one which may be considered as important as any other if not more important, and that is the examination of the new limb as worn for the first time by the patient.

A small volume might well be written on this aspect of the subject, and I can only touch on some of the points here. I am merely expressing an opinion, which may be wrong, but I feel that the surgeon's responsibility towards the patient has not ceased when he sends him, with or without a prescription, to the limb maker. It is not sufficient to note that the patient appears to walk satisfactorily. The case requires examination to discover whether the limb is suitable for the patient, whether it has been correctly fitted and correctly aligned, and whether the fitting is in accordance with the surgical needs of the case. If such an examination is not made a great responsibility is being thrown upon the maker which it is not right that he should bear. We are none of us infallible, but the maker, in spite of his experience, has not the detailed anatomical or surgical knowledge which can be employed in the interests of the patient. There are many patients wearing limbs which reflect credit neither upon the surgeon nor the maker, a misfortune which would be avoided by co-operation between the two. The patient may say he is comfortable, but he cannot know what is best for his stump; there may be good reasons for supposing that the comfort is only temporary.

His ability to walk correctly should be tested. He may require instructions; some he undoubtedly gets from the maker, but the amount depends upon various circumstances. The fitting should be checked to see that he really is taking his weight where and as required. The height of the limb should be checked, by placing the thumbs horizontally beneath the anterior iliac spines, having ascertained and allowed for any pelvic tilting. For a Syme's the patient should be level. For below-knees there should be one-quarter to one-half an inch shortening on the amputated side; for through- and above-knees a thumb's breadth shortening. Deviations from this may be permitted to a slight degree to allow for the conditions of the country in which the patient lives, hilly districts requiring more shortening than flat. Excess of either may produce lumbar curvature, and it is frequently necessary in the patient's interests to resist to some degree his own wishes in the matter.

When pelvic bands are worn the band should lie horizontally round the pelvis, and not be drawn up posteriorly, as one so often sees. The top border of the band should come just beneath the iliac crest anteriorly.

In rigid type bands the centre of the main screw of the hip-joint should be one quarter of an inch in front of the highest point of the great trochanter and at the same horizontal level, this representing the central axis of the natural hip-joint. Deviations from this will cause an internal or external rotation of the limb when walking, and the stump will tend to work against the limb.

It should be noted that in walking the limb swings properly beneath the patient, and does not describe an arc of a circle. In sitting there should be no adduction with internal rotation of the thigh portion as is seen sometimes in tilting tables, and which indicates either that the patient has not adjusted the limb properly, or that the artificial joints are in incorrect alignment. In below-knee limbs the swing of the leg should be noted ; there should be no rotation at the joints. The alignment of the steels should be such on the corset that, when sitting, the stump is not drawn out of the socket. These are but a very few points, the others can only be demonstrated, as it is most difficult to explain them in words.

Finally, in the world of prosthesis, as in other branches of our science, there is continual progress. Some excellent publications have appeared on the subject, but it has been difficult by literature to keep pace with the various changes. Such knowledge as we have is empirical, and is handed on by word of mouth to a great extent, since the subject is not yet stabilized.

Captain A. Rowatt Maxwell: Much of what we have already heard concerning amputations of the lower limbs and the problem of finding an efficient prosthesis from both a surgical and mechanical aspect, applies equally to the upper extremity. Owing, however, to their obvious natural differences in form and function, there must of necessity be certain surgical and mechanical variations between them, and when we discuss them from the prosthetic point of view it is advisable to consider them separately.

In the science of artificial limb fitting it is axiomatic that the task of providing an artificial substitute for an amputation of the upper extremity is an infinitely more difficult proposition than that of supplying a prosthesis for an amputation of the lower limb. The reasons are obvious. The arm and hand constitute a much more refined and complicated piece of mechanism than that of the leg and foot. The requirements of the latter are principally locomotion and weight bearing, while those of the forearm include flexion, extension, abduction, adduction, pronation and supination, and, greatest of all—the ability to grip. When we consider this organ of prehension—so delicate in anatomical design, so refined and perfect in coördination of muscle and nerve—is it any wonder that, so far, we have failed to evolve by mechanical means an efficient artificial substitute for the loss of it ? What then can we do by artificial substitutes to help make good this well-nigh irreparable loss ?

The Classification of Prosthesis for Amputations of the Upper Extremity.—Artificial substitutes, like those for the lower limb, are classified for amputations of the upper extremity. The classification is as follows :—

(*a*) For forequarter amputations.
(*b*) For disarticulation of the shoulder-joint.
(*c*) For amputations at the shoulder-joint.
(*d*) For short and long upper arm stumps.
(*e*) For disarticulation of the elbow-joint.
(*f*) For short and long forearm stumps.
(*g*) For disarticulation of the wrist-joint.
(*h*) For amputations of the carpo-metacarpal joints including even amputations of the digits.

In so wide a range of substitutes it must be evident that, from the point of view of practical utility, these artificial appliances are not of equal value.

Stumps of the Upper Extremity which may be regarded as Ideal for the Wearing of an Artificial Prosthesis.—My personal knowledge of this subject covers a period of more than thirty years, twelve of which have been spent at Queen Mary's Hospital

at Roehampton, during which about ten thousand arm amputés have passed through my hands. In expressing what in my opinion is an ideal stump for the wearing of an artificial prosthesis, it should be clearly understood that I do so principally from the limb-fitting point of view. An ideal stump should embody all those features that go towards making the wearing of an artificial limb not only possible but also successful. In other words, if the operating surgeon is to do the best for his patient, he should amputate with a view to the distinctive type of prosthesis with which the stump can be most perfectly fitted. This obviously presupposes on the part of the surgeon some knowledge of the various types of artificial limbs that are available. The acquisition of this knowledge will make the task of those who are responsible for the mechanical construction and fitting of the limb considerably less difficult, and in addition, if I may be allowed to say so, fewer re-amputations will be necessary.

There are three aspects of this great question of limb-fitting: (1) surgical, (2) mechanical, and (3) psychological. These cannot be divorced, hence the need for the closest co-operation between the surgeon who amputates and the persons who supervise the mechanical construction of the prosthesis and the accurate fitting of the same to the stump. It is no exaggeration to say that the results of such co-operation at Roehampton, and at our other limb-fitting hospitals, are proving of incalculable benefit to all concerned. It is also gratifying to know that in some of our civilian hospitals, doctors working for the Fellowship Diploma are being instructed in the various designs of these prosthetic appliances. This augurs well for the future, for, with the ever-increasing mechanization of modern industry, amputations of both extremities amongst civilian cases are steadily on the increase.

The Ideal Site of Election for Amputations of the Upper Extremity.—In amputations of the upper extremity, with which I am specially concerned, all things being equal, the ideal stump from the prosthetic point of view should be from 7 in. to 8 in. in length from the tip of the acromion process or, at least, 2 in. above the epicondyle, with equal anterior and posterior flaps, producing a transverse terminal scar.

From the limb-fitting point of view, the position of the scar in all amputations of the upper extremity is of paramount importance, and differs in some respects from the position desired in amputations of the lower limb. In the wearing of an artificial arm, particularly one designed for manual labour, the question of leverage and pressure plays an important part. At times the artificial limb is required to lift heavy loads; at other times considerable downward pressure has to be exerted. It will be readily seen, therefore, that the position of the scar must be situated neither anteriorly nor posteriorly, for, if it is, it will become subjected to considerable irritation, which is good neither for the scar tissue nor for the patient's comfort. Apart from the position of the scar, the ideal upper-arm stump should possess all those features that have been mentioned with regard to amputations of the lower limb.

For this ideal upper-arm stump several types of artificial limbs are available: (*a*) a strong working arm for manual labour; (*b*) a light worker's arm for sedentary occupations; and (*c*) for purely æsthetic purposes, a light dress arm.

The material from which the sockets are manufactured may be of metal, leather, wood, or a composition known as certalmid. The mechanism by which the arm is flexed, extended and locked varies in design. They are all constructed, however, to give the same results. In the production of artificial arms, standardization is impossible. Each has to be constructed individually in order to meet the special and peculiar requirements of the case.

The Ideal Stump for the Below-Elbow Amputation.—The ideal length of stump for amputations of the forearm should be not less than 6 in. and not more than 7 in. from the tip of the olecranon process. The scars, flaps, and other surgical features already mentioned in connection with the amputation of the upper arm apply equally to the below-elbow stump. For amputations below the elbow many excellent artificial substitutes are available and, when satisfactorily fitted, the forearm amputé can perform certain kinds of manual labour with a dexterity that equals that of many a two-armed man.

Some Difficult Types of Stumps to Fit.—Let me now indicate to you some sites of amputations of the upper extremity which, from the limb-fitting point of view, cannot be regarded as ideal, but which may nevertheless be provided with an artificial substitute: (1) The forequarter amputation. (2) Disarticulation of the shoulder-joint. (3) Through-shoulder amputation. (4) The short upper arm. (5) The disarticulation of the elbow-joint. (6) The short forearm. (7) The disarticulation of the wrist-joint.

(1) and (2). Apart from the forequarter amputation, of which happily we have very few (yet even some of these have been successfully fitted with a light type of prosthesis which conceals their disability), the disarticulation of the shoulder-joint is one that is very difficult to fit with an artificial limb, yet many of these cases have been satisfactorily fitted and have given astonishingly good results. To obtain good results, however, the following surgical features should be present. The position of the scar should be neither anterior nor posterior but vertically in the middle of the sub-acromial cavity. The axillary nerves should be free from pain, and the wall of the chest where the pressure of the moulded shoulder-cap is taken should be free from any condition likely to cause excoriation. The prosthesis for this amputation consists of a specially constructed protective cap covering the upper portion of the shoulder, including the clavicle anteriorly, and the spine of the scapula posteriorly. To this moulded shoulder-cap is attached, by means of a swivel-joint, the upper arm socket. The rest of the prosthesis includes the elbow-joint or side steels, and the elbow-locking mechanism, together with the forearm and hand. The whole of the prosthesis is secured to the body by means of leather or webbing appendages and thongs, giving automatic flexion, extension and locking. There is, I know, considerable prejudice against supplying an artificial limb for disarticulations of the shoulder-joint, but, where surgical conditions are favourable and the patient is willing and has the grit to try, much more can be done for this type of amputation than is at first apparent.

(3) *Amputations through the shoulder, where the neck of the humerus is intact, are much more easily fitted with an artificial substitute than are disarticulations of the shoulder-joint.*

(4) *The Short Upper-arm Stump.*—The short upper-arm stump includes those cases in which the length of available humerus is about 4 in. or less from the tip of the acromion process. This is not long enough without a modified shoulder-cap to control the ordinary upper-arm prosthesis. Sometimes, however, these short upper-arm stumps have been slightly improved by surgical interference, i.e., by dividing the pectoralis major, the latissimus dorsi and the teres major. It has thus been possible to utilize the axilla and secure an increased measure of leverage and control over the prosthesis. Such an operation is only advised when the amputé insists on the wearing of an artificial limb but is prejudiced against one of the through-shoulder type, and also where there is some degree of certainty that the stump can be lengthened by at least one inch.

(5) *The Through-elbow Type.*—From what I have said regarding the site of amputations for the upper extremity, namely, that the available length of stump should be sufficient to give adequate security and control over the prosthesis, it

would appear that the longer the stump, the greater will be the security and control thus desired. Yet, paradoxical though it may seem, we now come to consider one of those sites of amputation where this rule, from the limb-fitting point of view, is best observed in the breach. I refer to what is known as the through-elbow amputation, or disarticulation of the elbow-joint. The difficulties here, unlike those connected with the short upper-arm stump, are both surgical and mechanical. The surgical difficulties are adherent scar tissue, malnutrition of the skin and bony prominences. These, together with the mechanical disadvantages of an awkward, unsightly and ill-fitting prosthesis, predispose the stump to inflammatory processes and impaired vascularity, producing a cyanotic condition, and frequently necessitating a re-amputation. The type of prosthesis for this amputation is, in my opinion, mechanically the weakest and most unsatisfactory of all; not only is the socket unsightly because it has to accommodate the bulbous end of the stump, but the axis of the side steels have to be fixed at a level lower than that of the natural elbow-joint. The fixation of the elbow lock is lateral instead of being central and the awkward protuberance of the lower end of the socket makes it difficult to get the best use of the forearm when flexed at right angles. For this and many other reasons I am of opinion that through-elbow amputations or disarticulation of the elbow-joint are undesirable. As has already been pointed out, from the limb-fitting point of view, the ideal site of election is at least 2 in. above the epicondyle. Amputation at this site will preclude any possibility of the lower end of the stump fouling the elbow spindle, and will permit of the elbow mechanism, which gives automatic locking and flexion, being internally housed at the lower end of the socket, thereby giving greater mechanical strength and efficiency to the prosthesis as a whole.

(6) *The Very Short Forearm Stump.*—Let us now consider what is perhaps the most difficult and debatable of all sites of amputations of the upper extremity, from the prosthetic point of view. I refer to the very short forearm stump. It is here perhaps more than at any other site that I differ from my colleagues. Many surgeons regard, as practically useless, from the point of view of the fitting of an artificial prothesis, anything less than 3 in. of the ulna, yet in many cases in which the available length of stump has been less than 3 in. from the tip of the olecranon process giving not more than 2 in. anteriorly, we have been successful in fitting such stumps with a special type of prosthesis which has proved eminently useful. Of course, where the length of stump is not sufficient to control the artificial limb and secure it comfortably in the socket, then it is preferable to amputate higher up, at least 2 in. above the epicondyle. Where, however, security and control of the limb are at all possible, then be the length of the stump ever so small, its preservation is of the utmost importance. My own amputation stump is well within the 3-in. limit with not more than 2 in. anteriorly to control the socket, yet there is very little in the nature of manual labour—such as ploughing, digging, shovelling, wheeling, and the using of tools required for metal and wood work—which I am unable to perform. When you realize the vast source of energy locked up in these short ferearm stumps due to the insertion of the biceps, triceps and brachialis anticus, together with a matchless automatic fulcrum or back stop, where the upper end of the ulna butts up against the lower end of the humerus, which prevents the whole forearm, when fully extended, from bending backwards, you will readily appreciate how the conservation of such energy, where it is at all possible, is of the utmost value. The special type of prosthesis for manual labour which I recommend for these cases consist of an inner cup socket housed in a blocked leather socket which terminates immediately below the lower end of the stump, together with a metal rotary sector into which mechanical appliances can be fitted, a type of prosthesis which I designed for my own amputation in 1916 while serving

with the troops in France. This type of prosthesis is best secured to the stump by means of a Williams soft leather corset anteriorly and posteriorly laced above the condyles. For manual labour this type of prosthesis is as efficient as anything yet designed, it is simple but exceedingly useful, extremely well secured and easily controlled by the stump. Dissecting the socket immediately below the stump, enables the wearer to use his appliance as near as possible to his own muscular motor power, thereby giving him the maximum amount of leverage and control. Here, as in other sites of amputations of the upper extremity, the success of the prosthesis necessitates those surgical conditions that are indispensable to what we designate a good stump: freedom from scar tissue, neuromata, bony grating between the radius and the ulna, and from defective musculature due to other surgical causes or higher nerve lesions which might produce anæsthetic or hyperæsthetic conditions.

Kineplastic Amputations.—Before considering the disarticulation of the wrist, I should like to mention at this juncture the operation popularly designated "kineplastic."

I mention it here because, at one time, I was almost persuaded to have it done to my own short forearm stump. Indeed, most of the cases on which it was performed were somewhat similar to my own. True, many of them were stumps unlikely to make much, if any, use of a forearm prosthesis. Dr. Van Ghetty's idea was indeed a brilliant one, designed to utilize the muscles and tendons of the stump to operate the prostheses with which they were subsequently to be fitted. At the outset our hopes ran very high, and some of us believed that by synthetically creating artificial muscle by the problem of prosthesis for the upper extremity would be solved. Unfortunately, those hopes were not realized, not so much because the surgical technique was unsuccessful, but probably because a satisfactory mechanical hand could not be evolved. The movement or excursion of the muscular clubs or tunnels, when harnessed to the mechanism for opening and closing the hand, was very limited and produced a very feeble grip, nothing like so powerful as that which is so easily obtained by the ordinary appendages when harnessed to the muscular mobility of the trunk. As far as I know, none of the 30 or more patients upon whose stumps the kineplastic operation was performed are using the prosthesis with which they were originally fitted. Most of them have had re-amputations above the condyles, and have been fitted successfully with the ordinary upper-arm substitute. During a visit to the limb-fitting centres on the Continent, I found that in France, Belgium, Germany, and Austria, experience of the kineplastic operation, from the prosthetic point of view, was no more successful than our own in this country. When I was discussing this subject with Professor Spiezy in Vienna, he agreed with me that, until a satisfactory mechanical hand was evolved, no useful purpose would be served by continuing the operation.

(7) *Disarticulation of the Wrist-joint.*—It is not necessary to consider this at any great length. This operation has most of the disadvantages, both surgically and mechanically, that I have already mentioned in connection with amputations at the elbow-joint. One of the chief reasons, I presume, for performing this operation is in order that rotation at the wrist might be preserved. In these long forearm stumps, however, the movements of pronation and supination are the exception rather than the rule. Sometimes the loss is due to surgical causes, such as the union of the ends of the radius and the ulna, due to scar tissue or bony union. Frequently the loss of movement is due to the type of socket with which the stump is fitted, the socket fitting too tightly, thereby preventing rotation, or fitting too loosely, thereby allowing the stump to rotate within the socket. This type of amputation, from the limb-fitting point of view, is not desirable, not only because

the lower end of the stump frequently suffers from adherent scar tissue and malnutrition and takes unkindly to pressure, but also because the prosthesis is of an unsightly bulbous contour and on account of the rotary wrist piece being housed at the lower end, causing it to project 2 in. or 3 in. beyond that of the sound limb.

Note.—The only exception I would make in disarticulations at the wrist would be in the case of double-arm amputations in which the patients are remarkably successful with or without the use of appliances.

Mutilations of the Hand.—The stumps beyond the carpo-metacarpal joints are also exceedingly difficult to fit with an artificial substitute. When there is freedom from tenderness and pain on pressure, it is possible to provide a prosthesis, but in most cases it is preferable not to issue an appliance, but to leave the stump as it is because any part of the end-retaining sensitivity is of infinitely greater value than the most perfect artificial appliance ever invented.

Mechanical Hands versus Appliances.—Lack of space prevents me from dealing adequately with the vital and vexed question as to whether mechanical hands are more useful than appliances for amputations of the upper extremity. After having tested most of the mechanical hands in this country and studied the best of those available on the Continent, I have come to the conclusion that from the point of view of practical utility, appliances are superior to mechanical hands. The obvious weakness in most mechanical hands is their lack of power to grip. Most of them produce a feeble finger-and-thumb clasp, and even those designed to give a palm grip do so at the expense of immobilizing the wrist and thereby nullifying their usefulness. Our experience goes to prove that plain hooks, split hooks, claw hooks, spade-and-shovel grips and tool-holders of various designs are much more useful for manual labour than any of our mechanical hands.

The Value of Instruction in the Use of Artificial Arms and Appliances.—There are many interesting and important aspects of this subject with which, owing to lack of time, I have been unable to deal here. There is, however, one aspect upon which I must say something and with this I will conclude. I refer to its psychological side. When a man has the misfortune to lose his hand either through injury or disease, he is instinctively compensated with the prospect of being fitted with an artificial substitute, the possibilities of which he unwittingly over-rates. As soon, however, as he is fitted with his artificial limb, his whole mental attitude and outlook are changed and very often it is at this psychological moment that the success or failure of the appliance is decided. If he is discharged without some friendly instruction in the use of his arm and appliances, the chances are that he will discard these appliances as soon as he reaches home. If, on the other hand, he can be taken aside by someone suffering from a disability similar to his own and with some practical knowledge of the utility of these appliances and given some instruction in the use of them, the chances are that he will make good use of them in his efforts to earn a livelihood. For many years now it has been my privilege to impart such instruction. In addition to the elementary exercises of flexion, extension, abduction, adduction, the movements required in gardening, wood and metal work are also taught. This instruction is essentially functional; it is simple but sound and invariably alters the patient's whole attitude from despair to hope. It gives him courage and confidence because it enables him to overcome the initial defects in connection with the wearing of his limb.

In a social system where the race goes to the swift and the battle to the strong, the arm amputé is heavily handicapped but, given a good stump and an efficient prosthesis, his handicap, if it cannot be altogether abolished, can be reduced sufficiently to put him back again on the pay roll.

Such then is a brief outline of a very difficult problem, namely, that of providing

an efficient prosthesis for amputations of the upper extremity. It has a surgical side, a mechanical side and a psychological side and it is only when we work together in the closest co-operation that any measure of success will be reached. The problem is by no means solved, but it is considerably less difficult than it was twelve years ago.

Section of Orthopædics.

[*February* 4, 1930.]

Von Recklinghausen's Disease with Scoliosis.—W. T. GORDON PUGH, M.D.

The patient, a diminutive girl aged 15, whose father is stated to be similarly afflicted, says she first noticed the deformity of the back in 1928. She now has marked kypho-scoliosis centred in the mid-dorsal region, with multiple neurofibromata associated with areas of coffee-coloured pigmentation widely distributed over the body (figs. 1 and 2). The skiagram of the spine shows involvement of several vertebræ, and a wedge-shaped space between the sixth and seventh dorsal bodies, with its apex pointing towards the concavity (fig. 3). No tumours can be felt in the nerve trunks.

FIG. 1.

FIG. 2.

FIGS. 1 and 2.—Case of von Recklinghausen's disease with scoliosis. (Dr. W. T. Gordon Pugh.)

It has long been known that this disease is sometimes hereditary, and that occasionally it is accompanied by definite changes in the skeleton. In 1924 Brooks and Lehman, of St. Louis, described seven cases in which bone changes were found. Two were in mother and son, and the father of another patient had the disease in an advanced stage. The bony change in five cases was scoliosis; in four cases subperiosteal cysts were found. Brooks and Lehman thought that the cysts were probably due to the development of a neurofibroma in a nerve in the periosteum, causing a certain amount of bone destruction and regeneration, which had led to the tumour being covered with a thin shell of bone.

The degree of scoliosis is apt to become excessive, and it is suggested that in any case of neurofibromatosis in which scoliosis is commencing, a spinal fixation operation should be considered quite early.

FIG. 3.—Dr. Gordon Pugh's case of von Recklinghausen's disease with scoliosis.

Discussion.—Dr. PARKES WEBER said this case interested him from the ætiological standpoint. He believed there was scarcely any literature on what happened histologically in the scoliosis of von Recklinghausen's disease. Shortly after the war, Mr. Eric Pearce Gould had published an article on the subject in the *Quarterly Journal of Medicine*. Brooks and Lehman had suggested that the small ingrowths of neurofibromatous nature into the bones might possibly cause the weakening and bending of the spine.

He did not think it had been suggested that there was any treatment which would prevent the scoliosis; but fortunately, Recklinghausen's disease was itself very rare, or comparatively so, and few of the cases developed scoliosis. In the cases which he (Dr. Weber) had seen, scoliosis had not been present.

There was also another rare accompaniment of Recklinghausen's disease. He had recently jointly described a case of typical Recklinghausen's disease, with, in addition, an enormous swelling of one leg. An opportunity was afforded of examining this swelling, anatomically and histologically, and it was found to be due to a neurofibromatous thickening of the

periosteum over a considerable part of the shaft. He presumed that this attracted more blood to the bone beneath it, with the result that the cortical bone was much thickened.

The PRESIDENT said that he had been much interested in the peculiar appearance of the bodies of the vertebræ. As to treatment, he did not think that at this stage anything he was in the habit of doing would be of much use, because there was no bone substance. Whether bone fixation would have any effect in bone of this texture he did not know; he would hesitate to attempt it.

A Dwarf with Stippled Epiphyses.—ST. J. D. BUXTON, F.R.C.S.

C. S., aged 20, a dwarf; appears like an undeveloped lad of 14. He is one of five children, the others being normal in size and development, but a sister probably has tuberculosis of the lungs. An aunt of his father's is said to have been very short in stature. He went to school at the age of five, it being realized that he was then backward. He continued at the ordinary boys' school until the age of 15; the school kept him for the extra year as he was backward. He is not capable of doing work; in his mother's opinion he can do nothing beyond running errands. He is cheerful and honest.

There is no history suggestive of rickets or cretinism. Examination shows his short stature (height 46 in.), undeveloped genitalia, infantile voice, absence of hair on face, body and pubes. The length of limb in proportion to that of body is not abnormal. The hands are normal in shape. Slight genu valgum and coxa vara. No abnormality detected in urine.

It is not known what treatment he had previous to 1927, but he did not attend any hospital or doctor regularly. During the last three years he has had a small amount of pituitary extract and thyroid extract and calcium salts, but this treatment appears to have been irregular.

FIG. 1.--Skiagram of pelvis and upper part of femora showing stippling of epiphyses of femoral heads. (Mr. St. J. D. Buxton's case.)

(*a*) (*b*)

FIG. 2.—Skiagrams of elbow-joint (*a*) taken in 1927 showing stippling of capitellum; (*b*) taken in 1930 showing almost complete disappearance of stippling, but epiphysis has not joined the humeral shaft. (Mr. St. J. D. Buxton's case.)

FIG. 3.—Skiagram of knee-joint showing stippling of patella. (Mr. St. J. D. Buxton's case.)

Skiagrams have been taken of the greater part of the body. The main features are: Shafts of long bones shortened in length, with a normal proportion of breadth to length; no abnormality of the metaphyseal area. Great delay in union of epiphyses to the diaphyses. The epiphyses have a stippled appearance which is most noticeable at (1) the upper end of the femur (fig. 1); (2) the capitellar epiphysis of humerus (fig. 2 (*a*) and (*b*)); (3) the upper end of humerus.

The stippled appearance of the patella is very striking, resembling that of a mulberry (fig. 3).

The pituitary fossa is normal in size and outline. The Wassermann reaction, taken in 1927, was negative.

Stippled epiphyses occur in a number of skeletal diseases during the earlier years of life.[1] The author has failed to trace a similar case in the literature, but he himself had seen one other case.

Discussion.—Mr. H. A. T. FAIRBANK said that stippling occurred in the epiphyses in a number of different diseases. In the present case it was a developmental error, not necessarily congenital. It was present in dyschondroplasia. In some there was only an affection of the shaft, and in others the epiphyses were affected also.

[Mr. FAIRBANK then showed numerous skiagrams of different conditions, all showing stippling of the epiphyses.]

The PRESIDENT said that in most of the cases of which Mr. Fairbank showed skiagrams, the laying down of new bone at the epiphyses had been disordered on the diaphyseal side. In Mr. Buxton's case the stippling was finer, the epiphyseal shape had not been grossly disordered, and the end of the humerus was normal, except for the granular epiphysis. This placed it in a different category from the various bone-forming disorders in cartilage, to which different names had been given.

Sarcoma of Calf Muscles and Femur.—M. C. WILKINSON, M.B., B.S. (shown for Mr. W. T. GORDON PUGH).

Patient, a boy, aged 10, was admitted to Queen Mary's Hospital, Carshalton, on October 1, 1929, with a history of four months' pain in the right hip, which was held fixed by muscular spasm in slight flexion, adduction and internal rotation. There was also a lump in the left calf muscles, which had been present one month; no history of injury; the lump was hard and painless, being freely movable and not attached to skin or bone.

A skiagram of the calf, taken on admission, showed swelling of the soft tissues. That of the right hip showed slight destruction of the neck of the femur; the head was slightly flattened. The joint-space appeared to be normal.

The von Pirquet and Wassermann reactions were negative; nothing was obtained by aspiration from the calf.

The tumour of the calf slowly increased in size and on November 12 an exploratory incision was made by Mr. W. H. Trethowan; the fibres of the gastrocnemius were seen lying stretched over a tumour in the substance of the soleus; at one point the fibres of the gastrocnemius were invaded. The tumour was removed with as much surrounding tissue as possible; microscopical examination showed it to be a large round-celled sarcoma.

Successive skiagrams of the right hip show advancing rarefaction and disintegration of the neck of the femur, especially below the epiphyseal line; near the lower part of the neck (see figure), irregular ossification in the surrounding tissue has begun.

[1] Cf. H. A. T. Fairbank, *Brit. Journ. Surg.*, 1927, xv, 57.

Skiagram showing slight destruction of head of right femur. (Mr. Wilkinson's case of sarcoma of calf muscles and femur, p. 55.)

THE following cases were also shown: "Osteosclerosis of the Third Lumbar Vertebra, (?) Tuberculous, for Diagnosis."—GEORGE PERKINS, M.C., F.R.C.S.

"Charcot's Hip (?), for Diagnosis."—P. JENNER VERRALL, F.R.C.S.

[*April* 1, 1930.]

Two Cases of Cœliac Rickets.—P. JENNER VERRALL, F.R.C.S.

Out of a large family, six boys and three girls, only these two girls have been affected.

The elder, aged 14, has now recovered, with stunted growth, as present skiagrams show normal epiphyses. The only important residual deformity is a bilateral coxa vara (fig. 1).

Old—rather defective—skiagrams show that her condition was once exactly like that now present in her sister.

In the younger sister, aged 7, the disease is still active. She passes two large, pale grey, offensive motions daily and the epiphyses are in the condition shown in the illustrations (figs. 2 to 6).

I saw these cases in consultation with my colleague, Dr. Gardiner Hill, and the diagnosis does not appear to be in doubt, the only alternative one being that of mixed hyper- and hypo-chondrodystrophy.

The skiagrams show characteristic rarefaction of the bones, but few transverse striations in the diaphyseal ends.

Anti-rachitic treatment has been employed for some time with little effect. It is intended to admit this child to hospital and treat her for the primary disease.

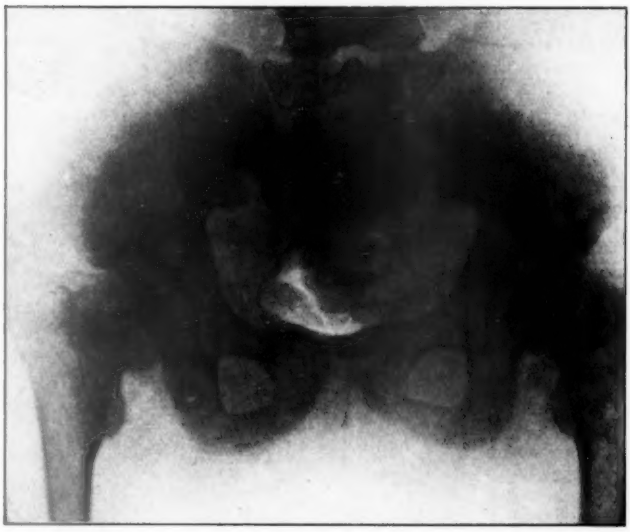


FIG. 1.—Bilateral coxa vara. (Mr. P. Jenner Verrall's first case of cœliac rickets.)

FIG. 2.

FIG. 3.

FIGS. 2 and 3.—Mr. P. Jenner Verrall's second case of cœliac rickets.

FIG. 5.

FIG. 4. Mr. P. Jenner Verrall's second case of coeliac rickets

I show these cases because I am under the impression that cœliac disease is under-rated by orthopædic surgeons as a cause of late rickets.

Dr. Alfred Hess, of New York, gives a good account in his recent book on rickets, in which he quotes Parsons as advising treatment by irradiated cholesterol. He also raises the, to us, very important point as to whether this condition should rank as a separate disease, as does renal rickets, or be merely regarded as a form of late rickets in general.

It is to be noted that the elder child has had no special anti-cœliac treatment, and possibly, therefore, the disease may be self-limiting.

Fig. 6.—Mr. P. Jenner Verrall's second case.

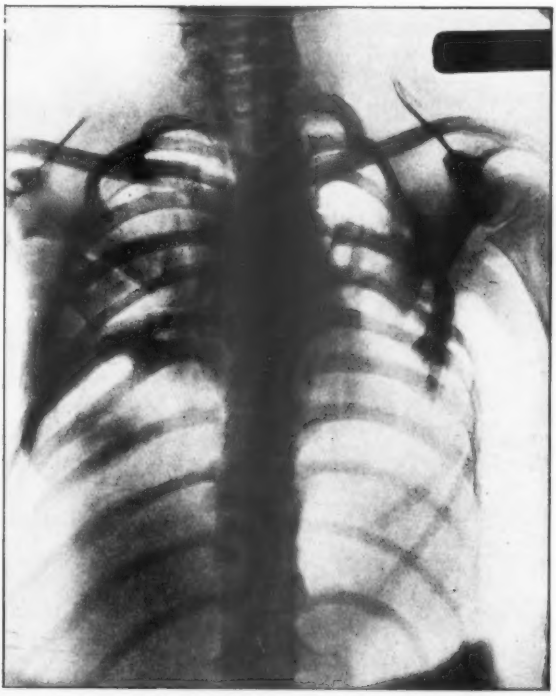
Bilateral Congenital Elevation of the Scapulæ.—H. P. Nelson, F.R.C.S.

Ida D., a small girl, aged 9, was brought to the hospital because of a bony deformity of the chest wall and an inability to raise the arms straight above the head. There was no family history of this condition.

Examination showed an elevation of both scapulæ and rotation of the inferior angle towards the mid-line. There was no bar connecting the scapula with the cervical vertebræ and the proportion of length to breadth of the scapula was

normal. There was an angular scoliosis, due to a wedge vertebra in the mid-thoracic region and a marked deformity of the chest wall, giving a prominence on the right side in front and a deep sulcus on either side, above the seventh ribs. Many of the ribs were fused and one was absent on the left side (see figure).

Treatment is being directed to increase the mobility of the shoulder-girdle by exercise.



Bilateral congenital elevation of the scapulæ. (Mr. H. P. Nelson's case.)

Discussion.—Mr. H. A. T. FAIRBANK thought these were unusually-shaped scapulæ in that they were deep. The tendency of the scapulæ in these cases was to shorten from above downwards, and their longest diameter was from within outwards. He also thought the angle was anchored on the right side much more than usual. The anchoring was usually towards the upper part, especially opposite the spinous process of the scapula. The one on the right seemed to be tied down near its lower angle. He had not before seen a case in which there was apparently a pointed process, curling upwards towards the spine in the direction of the rhomboid fibres and attached low down. In all the other cases he had seen there had been an attachment to the vertebral border opposite the spine. He would not operate in the present case.

The PRESIDENT said he did not regard this as a Sprengel's shoulder, or shoulder deformity at all. It was a spinal deformity and the scapular changes were adaptive. Two ribs were webbed together; sometimes three or four were webbed. He attached more importance to the congenital changes shown in the ribs and the spine. He had followed up one case in which seven ribs on one side were webbed together with bridges, and the expansion of the chest on that side was very defective.

Two Cases of Dislocation of Carpal Scaphoid.—S. L. HIGGS, F.R.C.S.

(I) R.S., male, aged 41.

History.—April 5, 1929: Motor-cycle accident (no further details given in notes). "Sprained" right wrist treated by massage. May 24: Seen by Mr. Elmslie at the the Royal National Orthopædic Hospital. A skiagram showed dislocation of scaphoid (fig. 1). June 19: *Operation* (Mr. Elmslie).—Incision over "snuff-box," scaphoid exposed and reduced easily. Splinted for three weeks with gentle massage and movements (fig. 2).

Examination.—Last examined on October 11, 1929: Wrist movements were good. No swelling. Forced movements painful. Abduction of thumb incomplete. To begin light work.

(II) J.F., male, aged 48.

History.—Three and a half weeks ago, while riding a motor-cycle, collided with a car travelling in the opposite direction. He thinks his right handle-bar struck the car. He was thrown heavily but has no recollection of exactly how he fell.

FIG. 1.—Showing dislocated scaphoid.

FIG. 2.—After reduction by open operation.

(Mr. S. L. Higgs's first case.)

Injuries to back of head, back and right wrist. Wrist remained very swollen and painful. Treated as a sprain.

An X-ray examination was made two weeks after the injury (fig. 3).

Points on Examination.—Obliteration of anatomical snuff-box. Lower end of radius cannot be identified. Extension of wrist to straight line only. Flexion through 25°. Slight ulnar deviation. Not very painful. Finger movements good but grip weak. Alteration of line of knuckles; 2nd and 3rd metacarpal heads less prominent and in same line with 4th and 5th metacarpals.

Suggested Treatment.—(1) reduction by manipulation only or by open operation, or (2) removal of scaphoid.

Discussion.—Mr. G. PERKINS said that in view of Mr. Elmslie's successful reduction, it might be worth while in Mr. Higgs' second case to put traction on the hand preliminary to open operation. A week's traction might open up the space and make reduction easier.

Mr. ALAN TODD said it would be a pity to spend a week on traction; he would operate at once in view of the time which had already elapsed. He would pull hard on the fingers as a preliminary, and endeavour to skid the scaphoid into position, if it would not go in without

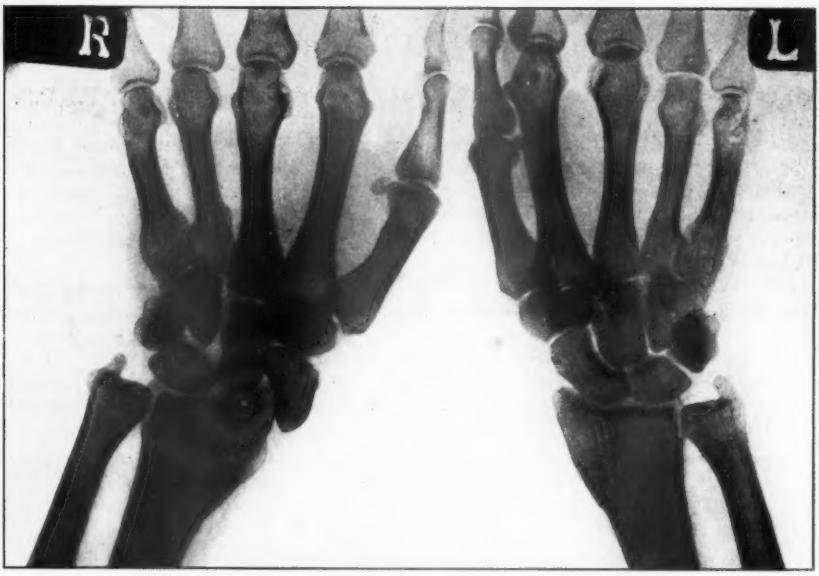


FIG. 3.—Dislocation of right scaphoid. Note upward displacement of os magnum and second and third metacarpals with consequent loss of prominence of corresponding knuckles. (Mr. S. L. Higgs's second case.)

FIG. 4.—After reduction of dislocated scaphoid by open operation. (Mr. S. L. Higgs's second case.)

skidding. He agreed with Mr. Higgs that the most important element in an operation on the scaphoid was to avoid injury to the rest of the joint space, particularly the articular cartilages.

POSTSCRIPT.—Operation note: The scaphoid went back into position with some difficulty but only after considerable opening up (fig. 4). The man now has a good grip and good movement, with very little discomfort (S. L. H.).

Swelling of Elbow, later proved to be Chondroma.—E. P. BROCKMAN, F.R.C.S.

Patient, a man, aged 45, has a swelling of the right elbow.

History.—Nine or ten years ago he noticed that he was unable to move his right elbow properly. This disability increased and he went to his doctor, but did not have any treatment as he was able to do his work. During the last two years he has had practically no movement in the elbow-joint, and it has become painful, having recently kept him awake at night. His general health has always been good.

FIG. 1.—Lateral skiagram of elbow-joint showing the large amount of calcification present in the tumour, which is eroding the posterior aspect of the lower end of the humerus. (Mr. E. P. Brockman's case.)

Present Condition.—Healthy looking man with a swelling of the right elbow-joint, which is almost completely fixed in mid-pronation and flexion to nearly a right angle. There is about five or ten degrees of movement in the joint.

The swelling is hard, in places almost bony in consistency and, in certain situations, tender. It extends down the forearm for about two inches up the posterior aspect of the humerus, and there is a large mass over the internal epicondyle. It is not an even swelling, but appears to be lobulated in parts, but is not attached to the skin, which is freely movable over it.

Skiagrams (figs. 1 and 2) show an extensive growth surrounding the elbow-joint, with multiple calcified areas, some erosion of the posterior aspect of the humerus and an obliteration of the joint line.

Wassermann reaction negative. No palpable glands in axilla of the affected side.

POSTSCRIPT (10.4.30).—A small incision was made over the posterior aspect of the olecranon, and a portion of the growth was removed for microscopical examination. This piece of growth was hard white cartilage.

Histological Report.—Areas of cartilage encapsulated. There is some superficial hæmorrhage and thickening of vessel walls. No evidence of malignancy.

Operation.—1.5.30: Through internal incision the growth was exposed. The ulnar nerve was freed, as this was surrounded by the cartilaginous tumour. The elbow-joint opened, and in order to get down to the humerus, the muscles, which were adherent to the tumour, though not infiltrated by it, had to be dissected off. The lower end of the humerus was excised after an osteotomy, and with it came away the largest mass of tumour, which had eroded the posterior aspect of the bone. The upper end of the ulna was removed with a further portion of the tumour, which

FIG. 2.—Antero-posterior skiagram showing similar condition to that in fig. 1

was infiltrating the bone, and some other portions of tumour were dissected out from the surrounding tissues. The incision was then closed and the arm put up in plaster.

The portions of tumour removed were all of the same consistency—hard cartilaginous tissue with, in certain parts, calcareous deposits which could easily be cut with a scalpel. The tumour substance eroding the lower end of the humerus could be scraped out with a spoon.

THE following cases were also shown:—"Maldevelopment of the Upper End of the Femur."—A ROCYN JONES, F.R.C.S.

"Bone Graft of the Ulna."—S. A. S. MALKIN, F.R.C.S.Ed.

Section of Comparative Medicine.

[*March* 26, 1930.]

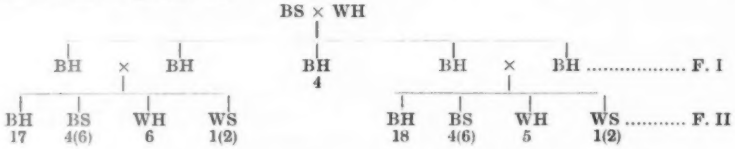
The Hereditary Transmission of Hypoglycæmia in Mice.

By P. J. Cammidge, M.D., and H. A. H. Howard, B.Sc.

We have elsewhere brought forward experimental evidence that a naturally occurring high blood-sugar level in the fasting blood of mice is transmitted as a recessive character in accordance with Mendel's theory of heredity (*Journ. of Genetics*, 1926, xvi, 387). We now propose to consider the results of subsequent breeding experiments with mice exhibiting the opposite condition of an unusually low blood-sugar level in the fasting state. For reasons to which we shall refer later, these investigations proved much more difficult to carry out, and our findings are consequently not so complete as could be desired, but information of considerable interest has emerged, as we hope to show.

In our previous paper we pointed out that hyperglycæmia is very frequently associated with another Mendelian recessive character, albinism, although our observations proved that they are genetically independent. In much the same way, hypoglycæmia is commonly, but not necessarily, associated with nigrism. The fact that uniformly black animals generally have an abnormally low fasting blood-sugar is, however, of considerable help in starting breeding experiments, especially when sufficient blood for a satisfactory sugar estimation can only be obtained after the death of the animal, as is the case with mice.

Experiment 1.[1]—A black mouse with a low fasting blood-sugar, of 60 mgr. per cent., was mated with an albino having a high blood-sugar, of 120 mgr. This resulted in a family of eight black mice, all of which had high blood-sugars, ranging from 116 to 120 mgr. (F. I.).

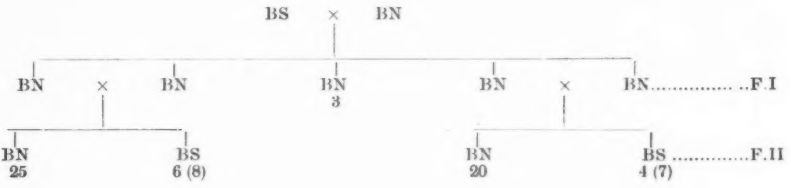


By crossing members of this family with each other a second generation of sixty-two mice was obtained (F. II). Of these thirty-five were black and had high blood-sugars, of 114 to 124 mgr., eleven were albinos with high blood-sugars, 110 to 120 mgr., eight were black with low blood-sugars, 60 to 62mgr., and two were albinos with low blood-sugars, 60 to 62 mgr.; four blacks and two albinos were born dead. These findings indicate that a low blood-sugar is recessive to a high blood-sugar, and also show that there is no linkage between the colour-factor and the blood-sugar factor,

[1] In the genealogical tables: B = black, A = albino, C = coloured, N = normal, H = high, S = sub-normal blood-sugars.

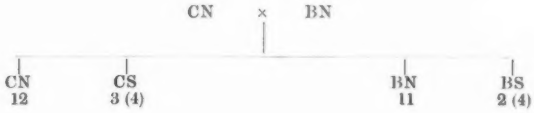
for they give a proportion of BH 35 : BS 8 (12) : WH 11 : WS 2 (4) where the expectation according to the Mendelian theory would be BH 9 : BS 3 : WH 3 : WS 1. The approximation of the actual results to the theoretical expectation is particularly close if the animals which were born dead are assumed to have had low blood-sugars.

EXPERIMENT 2.—A black mouse with a low blood-sugar (60 mgr.) was mated with another black mouse having a normal blood-sugar (80 mgr.), with a resulting family of seven, all of which were black and had normal blood-sugars, ranging from 78 to 82 mgr. (F. I). Interbreeding between the members of this family gave a second generation of sixty mice, all black. Of these 45 had normal blood-sugars (78 to 82 mgr.), ten were found to have low blood-sugars, 60 to 62 mgr., five were born dead and probably had low blood-sugars (F. II).



This gives a ratio of BN 45 : BS 10 (15), where BN 3 : BS 1 is the expectation if a low blood-sugar is recessive to a normal sugar.

EXPERIMENT 3.—A coloured mouse with a normal blood-sugar of 76 mgr. was mated with a black mouse having a normal blood-sugar of 78 mgr. These gave a family consisting of twelve coloured mice with normal blood-sugars, 78 to 80 mgr., three coloured mice with low blood-sugars, 56 to 60 mgr., eleven black mice with normal blood-sugars, 78 to 82 mgr., and two black mice with low blood-sugars, 60 mgr.; one coloured and two black mice were born dead.



This result suggests that the coloured mouse was heterozygous for colour and that both were heterozygous for sugar, as the Mendelian expectation under these conditions would be CN 6 : CS 2 : BN 6 : BS : 2.

EXPERIMENT IV.—A black mouse with a low blood-sugar of 60 mgr. was mated with two black mice having normal blood-sugars (76 and 80 mgr.). The resulting families totalled 103 surviving, all black, and forty, which were also black, born dead. Of the surviving mice seventy-seven were found to have normal blood-sugars (76 to 80 mgr.), and twenty-six low blood-sugars (60 to 64 mgr.), which gives a ratio of 3 : 1, but, as previous experiments had proved that the hypoglycæmic parent was a recessive, it could not possibly represent the true facts. If it is assumed that all the dead mice had low blood-sugars, the ratio would then be BN 77 : BS 66, which would suggest that the normal blood-sugar mice were heterozygous for sugar, since the expectation under these circumstances would be equality. Possibly unrecognized

abortions account for the disparity between the actual figures and the theoretical expectation.

Total ... BN : BS :: 77 : 26 (66). Expectation.—Equality.

EXPERIMENT V.—Numerous attempts were made to induce low blood-sugar mice to breed together, but with very indifferent success, as they were apparently sterile, or the progeny were either born dead or died so soon after birth that a satisfactory determination of their blood-sugar could not be made. We attempted to overcome the difficulty by feeding several pairs of low blood-sugar mice with daily doses of glucose, and found that their progeny survived for progressively longer periods as the glucose feeding was continued, as will be seen from the table, which relates one pair.

Family	Born alive	Died (number of weeks)					
		1	2	3	4	5	6
F. 1	7	5	2				
		Glucose feeding commenced.					
F. 2	5	0	3	2			
F. 3	6	1	0	0	0	0	5
F. 4	3	0	0	0	0	0	3
F. 5	6	0	0	0	0	0	6
F. 6	5	0	0	0	0	0	5
F. 7	6	0	0	0	0	0	6
		Glucose feeding discontinued.					
F. 8	5	5					
F. 9	6	6					

All the offspring proved to have low blood-sugars (60 to 64 mgr.) irrespective of whether the parents had, or had not, been fed on glucose; they also proved to be sterile and could not be induced to breed, even when they were fed on glucose. When the glucose feeding of the parents was discontinued all the progeny died as before, within the first week after birth, thus showing that a low blood-sugar is inimical to fertility.

Incomplete as these findings are, owing to the difficulties experienced in breeding from low blood-sugar animals, they indicate that hypoglycæmia, like hyperglycæmia, is transmitted as a Mendelian recessive character, and that there is no colour linkage, as appeared possible before our breeding experiments were carried out. The low fertility and high mortality among such offspring as are born alive of hypoglycæmic animals are very striking when compared with the large families of healthy descendants which can easily be obtained from high blood-sugar mice and from normal animals. That it is a deficiency of glucose, or a condition temporarily remedied by the administration of glucose, which interferes with conception and the development of the progeny seems to be definitely proved by the effect of glucose before and during pregnancy.

Clinical Forms and Bacteriology of Bovine Mastitis.

By F. C. MINETT, D.Sc., M.R.C.V.S.

THE various forms of mastitis may be classified, in accordance with the type of lesion, into parenchymatous, catarrhal, interstitial, suppurative, necrotic, or gangrenous. In some respects, however, it is more satisfactory to make subdivisions based upon the associated pathogenic bacteria. In this way there can be distinguished forms of bovine mastitis due to streptococci, staphylococci and other organisms. An account of our work on this subject, together with a résumé of the more important literature, has been published recently (1929) in collaboration with A. W. Stableforth and S. J. Edwards. In this investigation samples of udder secretion from 113 clinical cases of mastitis, not specially selected, were examined culturally, isolation being made in ox-blood agar poured plates. The predominant bacteria present were streptococci in 82, staphylococci in 6, *B. pyogenes* in 21, Gram-negative coliform bacteria in 3 and *B. necrosis* in 1. This list includes most—but not all—of the bacterial types which have been encountered in mastitis by other observers. Tuberculous mastitis was not included in this study.

Mastitis due to Streptococci.—Streptococci cause by far the largest proportion of cases of mastitis in cows in milk. This form is most prevalent in older cows, being usually of the chronic catarrhal variety, and resulting in slowly advancing fibrosis of the affected quarter, with corresponding diminution of milk secretion. In the early stages the only clinical evidence of infection consists of the presence of small flocculi in the first milk drawn, but at certain periods, e.g., just after calving and towards the close of lactation, the local symptoms often tend to become more acute, and the milk is then distinctly altered, being whey-like and containing large flocculent masses.

A second form of streptococcal mastitis has been observed, which in our experience, commonly occurs in young cows and even in first-calf heifers, which have been in milk but a few weeks. In this form acute local changes in the affected quarter are present from the onset, and these at times lead to rapid and total destruction of the milk-secreting tissue, with considerable atrophy as a result.

By cultivation on ox-blood agar plates fifty-two of the eighty-two strains of streptococci isolated were found to belong to Brown's beta-hæmolytic type, most of the remainder being green-producing forms of the alpha type. With one exception the former closely resembled each other in their cultural and biochemical characters. Growth in broth was flocculent and composed of long chains; litmus milk was acidified and clotted with reduction of the litmus in the lower part of the clot; acid was produced from glucose, lactose, saccharose, salicin (usually), and not from mannite, inulin and raffinose. In addition, with thirty-two of thirty-six strains tested high acidity was produced in 1% glucose broth at 37° C., the final pH after five days being 4·4 to 4·8. Thirty-seven of forty-two strains tested were capable of hydrolysing sodium hippurate to glycin and benzoic acid. Thirty-four of the fifty-two strains produced demonstrable hæmolysin when cultivated in 15% serum broth.

The characters of the thirty green-producing forms were more diverse, but by the cultural and biochemical reactions used it was possible to distinguish two broad sub-groups. Except for the type of hæmolysis in blood-agar, sub-group I—comprising fourteen strains—closely resembled the hæmolytic group. Strains belonging to the hæmolytic group and to sub-group I of the non-hæmolytic group agree in their characters with a fairly well-defined type, which has been referred to in the literature as *Streptococcus mastitidis* (Nocard and Mollereau) or *Streptococcus agalactiæ* (Guillebeau). In our experience strains of this type are responsible for the form of chronic streptococcus mastitis, which occurs so commonly in cows in milk.

The second sub-group could be distinguished by a greater tendency to short chain formation, and therefore diffuse growth in broth, by their appreciably lower acid production from glucose, lactose and saccharose and by lower activity in acidifying and clotting milk. Seven out of fifteen of the strains in this sub-group failed to hydrolyse sodium hippurate, and eleven out of thirteen failed to depress the pH of 1% glucose broth below 5%. This second sub-group is of special interest and deserving of further study. For one reason this variety of streptococcus appears to be responsible for many of the less common cases of mastitis referred to above as occurring in young cows. Further, some strains of this variety, when growing in liquid media, produce "toxic substances" capable of causing reactions on the human skin similar to those produced by Dick toxin. No statement can be made at present as to the nature of these substances.

A serological examination of mastoid streptococci is now being carried out by one of us.

Mastitis due to Staphylococci.—This form is of importance because, although apparently less common, the clinical course of the disease is frequently more acute than is usual with streptococcus mastitis and the animal is usually distinctly ill. There is a tendency for changes of a gangrenous nature to occur in affected quarters, with local sloughing in animals which survive. Cases, however, are frequently fatal; in the present series of six animals, two died, and a third had to be slaughtered. Five of these six cases occurred within the first few days after calving. In the more acute stages the secretion from the udder may consist of a thin, reddish-brown fluid somewhat resembling hæmolysed blood.

Culturally and biochemically the six strains isolated appeared to belong to a single group. Colonies on agar were pale fawn, acid was produced from glucose, lactose, saccharose and mannite, but not from salicin or inulin, litmus milk was rapidly acidified and clotted, gelatin was liquefied, deep colonies in blood-agar were surrounded by zones in which hæmolysis was incomplete. Strains were moderately pathogenic for rabbits and mice; one strain showed in rabbits a tendency to localization in the kidney.

B. pyogenes mastitis.—This organism is the most frequent cause of so-called "summer mastitis," a suppurative form which occurs principally in the summer months among dry cows at pasture. Nearly all the twenty-one cases in the present series occurred in dry cows shortly before calving and, with one exception, all cases were observed in the second half of the year. In seven cases constitutional symptoms were noted and in most instances the affected quarter was reported to have lost its function.

B. pyogenes belongs to the diphtheroid group of bacteria. Morphologically, the organism appears in the form of small, Gram-positive rods which are present in large numbers in the udder secretion. The organism is hæmolytic in blood-agar, possesses the power of acidifying and coagulating milk with subsequent digestion of the clot and is also capable of liquefying gelatin and solid blood-serum. The twenty-one cultures in the present series possessed these type characters.

Mastitis caused by Gram-negative Bacteria.—Of the three cases from which these bacteria were obtained, one was severe and showed systemic disturbance with subsequent loss of the quarter; the other two were mild. In the severe case, gas bubbles were noticed to be present in the secretion as it was drawn from the teat and the organism isolated was a hæmolytic variety of *B. coli.* The bacteria associated with the other two cases belonged to no well-defined type, but one strain in some respects resembled *B. proteus.*

Mastitis caused by B. necrosis.—Mastitis due to this organism has been rarely described. Necrotic lesions appeared in one quarter of the udder of a six-year-old cow and were accompanied by lesions in one of the carpal joints. The animal showed some systemic disturbance and during the few preceding weeks had lost

condition. The animal recovered, but six months later the affected quarter was greatly atrophied and had lost its function. The causal organisms were present in large numbers in the thick mucopus from the udder and from the affected joint. They were identified as *B. necrosis* by their morphology, by their strict anaerobism and by their pathogenic effects upon rabbits.

REFERENCE.

MINETT, F. C., A. W. STABLEFORTH, and S. J. EDWARDS, *Journ. Comp. Path. and Therap.*, 1929, xlii, 213.

Prophylaxis of Goitre in Fish Hatcheries.

By G. SCOTT WILLIAMSON, M.C., M.D.

TWO sets of workers are responsible for the present interest in thyroid problems. McCarrison was the first to stimulate interest by his studies on goitre in Gilgit. He showed, and has since I think made it conclusive, that one variety of goitre could be caused by some water-borne ætiological factor.

Following upon this, Marine and Lenhardt, and Gaylord drew attention to the so-called "carcinoma" of brook trout. These workers in a classical study of the conditions under which this disease arose fully confirmed McCarrison's work on ætiology. They showed this form of goitre to be acquired as an epidemic disease in the brood tanks. At that point in the study of the goitre problem, the epidemic variety of goitre under consideration was presumed to be identical with the endemic variety. We have shown since that these are distinct pathological entities. The endemic variety cannot be explained on the basis of the epidemic findings.

Marine and Lenhardt observed that the brook trout in the brood-tanks and streams acquired their goitres according to whether they were situated high up or low down in the stream. The lowest tanks had the highest incidence of enlarged thyroid. This was at once taken to mean that the cause of the goitre was an infection spread by contaminated matter flowing from the brood tanks above. McCarrison had come to the same conclusion in his study of the incidence of Gilgit goitre in its relation to the water supply drawn from different levels of the same stream. There arose, then, the theory of infection as the cause of this goitre, and some organism or parasite was said to be the cause. All the other chemical contaminations flowing from tank to tank were not, at that time, accounted as of any significance. For the time being both epidemic and endemic goitre were presumed to have bacterial infection as their cause.

Gaylord added an antiseptic—mercury compound—to the tanks, and in this way the goitre ceased to appear in the younger broods. Marine and Lenhardt added iodine to the tanks with the same result. Neither of these compounds could have been in sufficient concentration to have acted as antiseptics. Marine and Lenhardt apparently took this view, for Marine proceeded to associate the iodine with some specific effect. This, indeed, seemed likely, for iodine was known to form a part of thyroid secretions. He proceeded from this to a study of endemic goitre, and revived the old theory of iodine deficiency again as the cause of both goitres. This theory received support from the fact that iodine (and all other measures adopted) failed to influence the existing goitre in the brood tanks or in the older fish. Iodine was not a cure; it was a prophylactic. This has been a most prolific theory, and has quite illegitimately included every type and variety of goitre in its promiscuous embrace.

McCarrison has shown that there are experimental goitres which are produced in the face of iodine abundance: i.e., lymphadenoid goitre and hypertrophic goitre.

Further, from his later studies it is clear that "dirt," if not infection, is a potent cause of goitre under special conditions not related to iodine deficiency, though iodine, naturally enough, can affect these goitres to some extent since it is, after all, one of the important products of thyroid activity. It is impossible to argue safely from therapeutics to ætiology.

The fish goitres were furthermore shown to be hypertrophies and not inflammations. It is still difficult to understand why a prophylactic measure should not also cure a hypertrophy when any deficiency is made up. There were now two theories, one that iodine deficiency was the cause of goitre and the other that an infection was the cause of goitre. I think myself it is quite easy to reconcile one theory with the other, for an intestinal inflammation could conceivably create a deficiency by acting on the threshold of absorption in the gut. Thus the colloid goitres of endemic zones could be a direct deficiency and the hypertrophic goitre of epidemic nature an indirect deficiency disease.

Goitre in brook trout-hatcheries occurs in this country. It is not closely associated with endemic zones. It crops up at odd times and unexpectedly. It is not dependent, so far as is known, upon the change in any procedure in the hatcheries. It is in fact epidemic. The present interest lies in the methods of preventing the goitre, worked out empirically by those in charge of the hatcheries.

Two methods are in use. One is the addition of Condy's fluid to the tanks. This is comparable to treating the water in the tanks just as Gaylord and Marine did with very small doses of some active chemical. In this case it is with a substance which is said to be antiseptic.

Our present interest is with the second method, however. A spread of goitre is said to be prevented by treating the young fish and not the water in the tanks. When the goitre spontaneously appears in these hatcheries all the younger fish which have not yet acquired goitre are removed from the tank. They are soaked in a pail containing a great deal of common salt (¼ lb. to 2 gallons). The young fish "go dead" as they say: become comatose I suppose. One is warned not to be alarmed at this state of affairs. While in this comatose state they are returned to the tank and they slowly recover. Goitre ceases to appear in the tank. This procedure is useless as a cure for any established goitre.

It is very difficult to understand how soaking in salt can serve as a substitute for iodine, Condy's fluid and mercury compounds. The salt is of hypertonic strength, the other antiseptics are in hypotonic solution. The salt may have contained traces of iodine, permanganate, or mercury.

On the other hand, the thyroid gland may be concerned in mineral salt balance, of which iodine only forms a small part, however essential that may be. It is to that probability that I wish to draw attention.

Section of Epidemiology and State Medicine.

[*May* 23, 1930.]

Infectiousness and Immunity in Regard to Chickenpox, Whooping-cough, Diphtheria, Scarlet Fever and Measles.

By Percy Stocks, M.D.

(Reader in Medical Statistics in the University of London.)

Introduction.—A survey of the pronouncements of epidemiologists as to what is the most important factor in the control of epidemics leaves one with the impression that these exhibit to a remarkable degree at least two of the characteristics of the phenomena of which they treat, namely, variability and periodicity. I do not propose to follow the well-trod path back to Sydenham to prove that there has been a succession of cycles in which the emphasis has alternated from the infecting agent—subject now to its biological modifications, cycles of development, or changes in virulence, and now to the mysterious influences of epidemic constitutions, through obscure atmospheric and seasonal states—to the attacked population, with its changing susceptibilities and immunities, and then back again to the virus, for which new and remarkable properties have in the meantime been postulated. The last swing of the pendulum seems of recent years to have brought us very rapidly and unmistakably again to the viewpoint that immunity is the most interesting and perhaps the most important of the factors concerned in the mechanism of epidemics.

For this we have to thank the combined labours of investigators in several different fields. The field work of Glover [1] on the behaviour of cerebrospinal fever and the careful cartographical work of Wickman and Wernstedt [2] on the Swedish epidemics of acute poliomyelitis in 1905 and 1910-13 indicated that in the control of epidemics of these diseases at least immunity is the most important factor. The observations of public health workers who have been applying the immunological methods evolved by Schick and the Dicks have proved almost beyond doubt that processes of latent immunization are responsible for protecting very large sections of the population against diphtheria and scarlet fever, and the careful and ingenious researches of Dudley [3] on the behaviour of a small enclosed population have thrown much light upon the mechanism by which this is brought about. The team work of the experimental and statistical epidemiologists, Topley and Greenwood [4], have also proved that immunity acquired, without contracting the disease, by exposure to infection from pasteurellosis and mouse typhoid is an important factor in the control of epidemics of those diseases in populations of mice. Finally, the work of Gill [5] on the spleen rate in populations exposed to epidemics of malaria has brought to light evidence of wholesale immunity acquired during epidemics by natural processes, without clinical illness, which then gradually subsides before another epidemic can occur.

In considering the periodicity so characteristic of measles in urban areas, which had furnished such fascinating material for study at the hands of Munro [6], Whitelegge [7], Hamer [8] and Brownlee [9], Gill's work suggested to me that a process of latent immunization, not permanent as seems to be the case in diphtheria, but slowly disappearing as in malaria, might explain why the amplitude of rise and fall in the proportions of children protected by a previous attack of measles in a population is too small to be regarded as the chief cause of the periodicity. That it is too small was proved by a laborious analysis of the notification records in St. Pancras, and in support of the theory of a temporary latent immunization of the

population gradually lost again over a period of two or three years, I was able to publish some evidence more than a year ago [11] and to amplify it in a more recent paper [21].

And if latent epidemization be an important factor in diphtheria, scarlet fever, cerebrospinal fever, acute poliomyelitis, malaria and perhaps measles, might it not also be of importance in whooping-cough, smallpox, chickenpox, German measles and mumps?

In taking up this line of study it was of course obvious that there were great difficulties ahead, because the only method of measuring immunity of individuals to this group of diseases at present is by observing whether these individuals do or do not contract the diseases when exposed to infection in the ordinary course of events, that is to say, the weapons of research in this field are purely statistical. Reliance must also be placed upon the data made available by the compulsory notification of these diseases in certain local areas. In districts such as St. Pancras and Paddington, where compulsory notification of measles has been in force for years, I have satisfied myself that notification data are free from any bias likely seriously to influence statistical conclusions, except in two respects, which must be kept in mind: (1) that a possible margin of about 15%[1] must be allowed for those who escape notification, and (2) that there is some inevitable confusion with German measles during epidemics of that disease.

A careful analysis of histories in regard to several infectious diseases has recently been made by Collins [13], based upon very large numbers of school children, college students and others in fifteen localities in the United States of America. In this paper catalytic curves have been fitted to the data after division into years of age, and these show that 89% of children have measles, 52% have chicken-pox, 11½% have scarlet fever and 10% have diphtheria, on the average, in those localities by the seventeenth year of life. Allowing for German measles in the total it seems a fair conclusion that about 85% of children in the mixed population of a large town have measles at some time during life.[2] A calculation from the records in Paddington later in this paper shows that about 40% of children born are likely to be notified for chicken-pox, and comparison with the figure of 52% obtained by Collins from histories suggests that the notified and reported cases in Paddington represent the bulk of the cases occurring.

Statistical evidence for latent immunization—methods of approach.—There are two methods by which immunity changes in a population may be studied by means of notification records: (*a*) by measuring the apparent infectiousness of cases of the disease at different periods of time, and (*b*) by actually following up contacts intimately exposed to infection and comparing their subsequent behaviour with control groups not so intimately exposed.

(*a*) *Apparent infectiousness.*—In his Milroy lectures in 1906 Hamer [8] made an analysis of the London measles wave, which was a delightful example of the application of simple mathematical processes to epidemiology, carried out with such clearness of exposition that a child could understand it. In this he supposed that the measles cases occurring in a given week arose directly from those occurring in the week but one previously, and pointed out that near the start of an epidemic wave four cases were giving rise to five on this basis, whereas near the end five were giving rise to four, and on the assumption that this change was entirely due to the reduction of susceptibles in the population he proceeded to equate these changes to the cases during the epidemic and the influx of susceptibles by births, and thus arrived at an estimate of the numbers of susceptibles in the

1 I have shown [11] that in St. Pancras 70% of children are notified for measles before adult life, and since German measles is separately recorded, about 75% will be notified for one or other disease.

2 Butler [12] concluded that in Willesden about 97% of children attending public elementary schools had measles at some time, but this figure, as he admitted, was only applicable to the class of children selected, and it included, I think, German measles.

population. In the steady state, where weekly cases are neither increasing nor decreasing, as in an inter-epidemic period, one case infects one on these assumptions, and the fractions $\frac{5}{4}$ and $\frac{4}{5}$ might be regarded as indices of infectiousness, and, indeed, they have really been used as such in a recent more elaborate mathematical examination of Hamer's original assumptions in a paper by Soper to the Royal Statistical Society [10].

When we pass to Brownlee's [15] mathematical analysis of the epidemic curve, we find again the use of the incubation period as interval, and the assumption that all cases contracted their infection from other clinical cases which occurred at this interval further back in time; but the effect of changes in proportions of susceptibles in the population is now neglected, and the infectiousness, as it is evidenced by the rate of increase of cases, is equated to a rate of loss of infectivity of the infecting agent. The discovery of the importance of carriers and latent cases in infectious diseases makes it probable that the assumption made by both Hamer and Brownlee, that every clinical case arises directly from a previous clinical case, is not at all a safe one, even in general reasoning of this sort, for infection may be handed on by carriers or latent cases, which may be more numerous in proportion to actual cases at one period of the epidemic cycle than another. This assumption would certainly lead us astray in dealing with cerebrospinal fever and diphtheria, and it may also do so with measles and other diseases. It is also evident that one or both of the opposing assumptions, that the infecting power of the virus does not change, and that changes in this infectivity are all important, must be wrong. Thus far, then, no satisfactory measure of infectiousness had been devised, and the proposal of Stallybrass in a paper read before this Section [14] to introduce the term "index of dispersability" did not solve the difficulty, for it was once again the ratio between numbers of cases arising in successive intervals of time, which now had no relation to the incubation period.

An index of infectiousness was wanted which, whilst avoiding the assumption that there is no reservoir of infection other than clinical cases, and allowing for changes both in the facility of transmission from case to case, if this is subject to change, and in the proportion of non-immune persons present in the population, would at the same time be sufficiently simple to calculate to make it of practical use to public health workers not trained in mathematics. In my paper on the "Epidemiology of Measles" I defined such an index and termed it the *index of apparent infectiousness.*[3] It involves the definition of a "secondary case" for the purposes of the index, as a child of a defined age (A) who develops the disease at a defined interval of time (T) after a case (of any age) residing in the same house, the interval T being chosen to embrace most of the possible range in the incubation period. As an example, for measles A has been taken to include all ages under 10 years, and the interval T to be ten, eleven, twelve or thirteen days between appearance of the rash in the two cases, the reason for this selection being made clear in the distribution of 3,450 such intervals in Table 1. Where a case arises *in the same house* ten to thirteen days after a previous case, the probability that it was not due to direct infection but to the agency of some source of infection outside the house, may be regarded as so small as to be quite negligible, although the probability that the first case arose from a carrier and not a clinical case may be quite considerable. By thus standardizing the conditions of exposure by restricting them to those involved in residence in the same house as a case, the difficulty of the possibility of important reservoirs of infection existing other than clinical cases is got rid of, and moreover it enables us, as I shall show, to obtain an index of infectiousness far more sensitive than Hamer's ratio.

With this as definition of a "secondary case" the apparent infectiousness during any given period is measured by the ratio p of the number of secondary cases[3] s arising from them to the total cases n occurring during the period. Whether these

[3] For a full exposition see Reference No. 11, pp. 389-390.

secondary cases all occur within the period studied or not is immaterial; the infection may be assumed to have been transmitted to children living in the house within a few days of the onset of the first case, and it is the intensity of this infecting process which is being measured, so that the dates of onset of the secondary cases need not be considered so long as the interval is right. Thus the infectiousness during the first week of March for measles would be given by the total cases developing the rash in that week divided into the secondary cases to them, which, according to the definition, might develop the rash on any date from March 11 to March 20, but would presumably have received their infecting doses during the first week. This ratio $p = s/n$ being dependent upon such local factors as average size of family and conditions of overcrowding, in order to obtain an index comparable from one area to another, the ratio is best expressed in terms of its value p_o during a post-epidemic period, when incidence is steady and at a low level, as unit.

The resulting *index of apparent infectiousness* p/p_o is the product of two other indices not so readily measured, namely the *index of true infectiousness* or relative facility of transmission from person to person [4], which is subject to sudden fluctuations as we shall see, and the *index of communal susceptibility*, which is the proportion of the population aged A susceptible to the disease at the middle of the period in terms of the proportion susceptible at the middle of the standard post-epidemic period.

To take an example, suppose that in a town of 600,000 population, of whom 100,000 were children under 10, there occurred in the month of January in an epidemic year 1,000 cases of measles, and that in the houses where these occurred there followed 120 cases aged under 10 with an interval of ten, eleven, twelve or thirteen days between the appearance of the rash in the two successive cases. Then 120/1000 or ·12 will represent the apparent infectiousness p during January, and in order to turn this into an index we express it in terms of p_o for a post epidemic period when incidence was stationary. For this purpose it is advisable to choose a longer period than a month, since the numbers of cases per month in such a period may be small and that of secondary cases will be still smaller. Let us suppose, then, that in the four months August to November there occurred 500 cases, followed by twelve secondary cases as defined, then 12/500 or ·024 represents the apparent infectiousness p_o, and using this as unit we find the index of apparent infectiousness p/p_o in January to be ·12/·024 or 5 exactly. This means that the probability of being followed by another case aged under 10 in the same house after the specified interval was five times as great at the beginning of the epidemic as in the quiet period following the epidemic. Now part of this is certainly due to a greater proportion of the children exposed being susceptible in the first instance than in the second. Let us suppose that 75,000 of the 100,000 children were susceptible in mid January, and only 30,000 remained susceptible at the middle of the post-epidemic period, then the *index of communal susceptibility* in January would be ·75/·30 or 2·5, and this would account for a $2\frac{1}{2}$-fold increase in the apparent infectiousness, whereas a fivefold increase was actually found. We should then have to assume that the remainder of the increase was due to a real increase in the facility of transmission of the virus from case to contact, which I have termed the *index of true infectiousness.*

The equation connecting the three indices is the simple one:—

$$\text{Index of apparent infectiousness} = \text{Index of communal susceptibility} \times \text{Index of true infectiousness}$$

and in the example given the index of true infectiousness would be 5/2·5, or exactly 2, implying a twofold increase in the facility of transmission of the virus at the start of the epidemic as compared with its value in the post-epidemic period.

The index of apparent infectiousness is very easily measured by merely sorting the notification forms into alphabetical order of streets, bringing together all pairs of cases occurring in the same house with the specified interval between the onsets and

[4] This is really what Brownlee called "infectivity," and some have called "virulence."

with the specified age of the second case, and it has the advantage that it can be calculated for any period whatever according to convenience, which need not be restricted to calendar months or other arbitrary units having no relation to the epidemic phenomena. The behaviour of this index, considered in relation to the epidemic curve, can by itself tell us something about what is happening to the factors of which it is a product, for we can safely, I think, make certain inferences such as: (1) that the immunity of a population does not change in a sudden or explosive manner, and hence such sudden fluctuations in the index as are statistically significant must be attributed to changes in facility of transmission,[5] and (2) that a gradual rise in the index continuing over years without reference to seasonal influences is almost certainly due to changes in the immunity index. In order to obtain a reliable quantitative measure of the changes in the two component factors it is necessary to supplement the information secured from the composite index by some other methods of measuring immunity changes in the population, which will now be described.

(*b*) *Immunity of contacts.*—There are several possibilities to be considered: (1) that immunity is only attained by a clinical attack; (2) that an appreciable proportion, y, of children born, retain an inherent immunity to the disease; (3) that there is a latent immunization of a permanent character of x persons on the average to every notified case; (4) that there is a latent immunization of a temporary character rendering x persons immune to every notified case in the first instance, but then disappearing according to some continuous law of decrease; or (5) a combination of (2) with either (3) or (4).

In the first of these events the changes in communal immunity can be directly measured by deducting notified cases (corrected if necessary for incomplete notification) and adding acquisitions by births, the index of communal susceptibility calculated and the other component index thus arrived at indirectly. The presence of inherent immunity must be inferred rather than proved if, having ruled out latent immunization by one of the methods which follow, it is found that contacts have no greater ultimate expectation of catching the disease than children of the same age in the population as a whole, meaning that a certain proportion of children born are destined to have the disease, if not sooner, then later. In the second or third events, if y or x can be estimated, the changes in communal immunity can be arrived at, in the second event, by deducting notifications and adding $(1 - y)$ times the survivors of the births, and in the third event by deducting $(1 + x)$ times the notifications and adding the survivors of the births. In the fourth event the question is more complicated, but if some simple law of decrease in the number of latent immunes remaining after the lapse of a given time can be assumed or deduced, it is not at all difficult of solution provided that the rate of disappearance r can be estimated.

The methods I have begun to use with a view to settling whether there is any latent immunization at all, and if so whether it can be regarded as permanent or temporary and what are the values of y, x, and r characteristic of each disease, consist in following up through the records of the schools and health departments contacts of certain ages known to have been exposed to infection from a case in the same house, and comparing the rates at which they fell victims to the disease during subsequent intervals of time with the same rates of attack in control groups of children living in the same area or attending the same schools. Several methods have been used, all based upon this principle; thus the control group may consist of all children at risk of the same ages in the general population, or of children of the same ages who have been contacts to some other disease. In order to combine all ages together, a method of standardization for age by applying a set of smoothed

[5] If evidence of this is thought necessary, a study of fig. 5 in Reference No. 21 will show that the sudden increases preceding the epidemic wave cannot represent immunity changes in the population attacked.

attack-rates at ages has been employed, and where necessary a correction applied for children lost to view by estimating the annual removal-rate by means of the annual deductions from the electoral register.

These methods will be illustrated in the work I shall now describe, but the results so far obtained must be regarded as only tentative and introductory to further work on the same lines.

Diphtheria and scarlet fever.—The changes in apparent infectiousness of diphtheria and scarlet fever in St. Pancras during the five years 1925-29 have been worked out for comparison with the other diseases. For access to the records I am indebted to the Medical Officer of Health, Dr. G. Sowden. The distributions of intervals between the onset of the disease in pairs of cases occurring in the same house are shown in Table I, and the range of intervals chosen for definition of a "secondary case" was three to twenty days for diphtheria and two to eleven days for scarlet fever, the age of the secondary case being restricted to under fifteen years in each instance.

TABLE I.—INTERVALS IN DAYS BETWEEN ONSET OF DISEASE IN PAIRS OF CASES OCCURRING IN SAME HOUSE

Days	Diphtheria (St. Pancras)	Scarlet fever (St. Pancras)	Measles (St. Pancras)	German measles (St. Pancras)	Whooping-cough §		Chickenpox (Paddington)
					Wandsworth	Holborn	
1	24	11	341	27	5	1	116
2	18	25	246	7	8	3	93
3	15	31	160	8	27	5	67
4	19	14	117	3	33	7	44
5	21	16	96	—	36	10	33
6	15	8	90	4	37	13	24
7	11	8	99	6	37	14	39
8	12	15	173	—	36	13	31
9	12	11	206	2	34	12	39
10	8	7	318	2	32	11	45
11	4	8	353	3	30	10	64
12	6	2	329	3	29	10	106
13	2	4	269	7	27	9	205
14	8	1	205	6	25	9	260
15	4	6	153	8	24	9	222
16	5	4	104	5	22	8	148
17	5	3	70	8	21	8	63
18	—	4	48	4	19	8	65
19	2	2	38	6	18	7	28
20	4	3	35	3	16	7	23
21	—	3	*	1	15	7	30
22	2	4	*	4	13	6	13
23	—	1	*	—	11	6	7
24	1	3	*	1	8	5	9
25	1	1	*	1	6	4	7
26	1	1	*	—	4	4	15
27	1	3	*	—	3	4	7
28	6	2	*	—	2	4	13
†	56	43	*	1	26	36	124

§ Smoothed distributions. † Four to twelve weeks. * Not analysed.

The notified cases of diphtheria during the five years numbered 1,806, and the secondary cases as defined numbered 121.

From December 29, 1924, to August 30, 1925, there were 335 cases in thirty-five weeks, or about ten per week, and these gave rise to thirty secondary cases, a ratio of 8·96%. In a period of lower incidence and low infectiousness, from August 2, 1926, to January 30, 1927, there were 195 cases in twenty-six weeks, or seven and a half per week, and these gave rise to only seven cases, a ratio of 3·59%. The difference of 5·37 ± 1·39 may be regarded as significant,[6] and the index of apparent infectiousness has been calculated for consecutive periods throughout the five years in terms of the value 3·59% as unit. The result appears in fig. 1, which shows also the mean cases per week in each calendar month. When the records begin in 1925 the index was 2·50 (viz., 8·96/3·59) with an incidence of about ten or eleven

CHANGES IN APPARENT INFECTIOUSNESS OF DIPHTHERIA AND SCARLET FEVER
ST PANCRAS. 1925-1929.

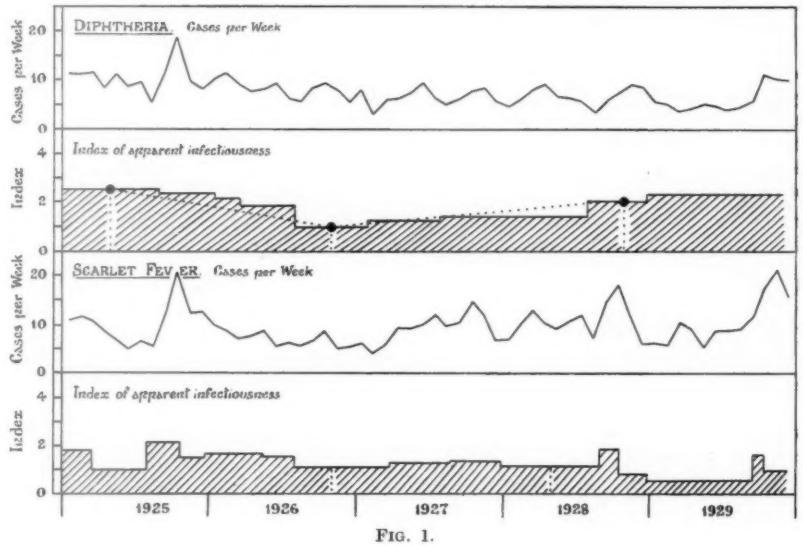


FIG. 1.

cases per week. A small autumn "outbreak" followed, with an incidence during October exceeding eighteen cases per week, and both incidence and apparent infectiousness then progressively declined to the autumn of 1926. During the next three years incidence remained low, averaging 6·3 cases per week, but the index of apparent infectiousness rose steadily from unity in the period August 2, 1926, to January 30, 1927, to 2·02 in the period July 30 to December 23, 1928, and to 2·27 during 1929. During the last three months of 1929 incidence was beginning to rise again, averaging eleven cases per week.

The first thing to notice is that no sudden fluctuations are evident in the index, such as we find in the other five diseases, but only gradual changes. The data here worked out are not nearly extensive enough to prove that such sudden changes,

[6] These ratios relate only to the universe of affected houses, and the secondary cases which occur within this universe are not appreciably correlated with each other owing to their definition, hence the ordinary formula for random sampling has been used, here and subsequently in this paper. This could not, of course, be applied with safety to ordinary rates of incidence of an infectious disease.

occasioned, as they must surely be, by changes in true infectiousness rather than communal immunity, do not occur in diphtheria, but the contrast with the other diagrams suggests that they are not characteristic, and that this may be the reason why epidemics of an explosive character are also not characteristic of diphtheria. We may infer that the gradual changes in the composite index during 1925-29 were chiefly due to changes in the index of communal susceptibility. Let us assume they were entirely due to this and see where this leads by a simple calculation, proceeding from the facts indicated by the dotted lines in fig. 1, that the index was (*a*) two and a half times as great on May 1, 1925, as on November 1, 1926, and (*b*) twice as great on November 1, 1928, as on November 1, 1926, these dates being the middle of the periods for which the index was computed. During the first interval, whilst the communal susceptibility was falling, there occurred 746 cases in one and a half years, and during the second interval, whilst it was rising, there occurred 680 cases in two years. Since we know that latent epidemization must occur in diphtheria, let us suppose that each notified case was accompanied by x latent cases, and let M be the number of susceptibles on November 1, 1926, in the population at risk to attack by diphtheria during these years. The susceptibles were being replenished by fresh births, and we may arrive at perhaps the best rough approximation to this replenishment during any year, by deducting from the births in the previous year the deaths under one year of age, giving the survivors to one year who were being added to the population. On this basis the susceptibles added to the population during the first interval of one and a half years were 5,480, and during the second interval of two years 6,450, and if we assume that the whole population and all children born to it were at risk to infection, we obtain :—

(i) $746\,(1 + x) - 5{,}480$ = loss of susceptibles from May, 1925, to October, 1926 = $2\frac{1}{2}\,M - M = \frac{3}{2}\,M$.
(ii) $6450 - 680\,(1 + x)$ = gain in susceptibles from November, 1926, to October, 1928 = $2\,M - M = M$.

and equating these to eliminate M we get :—

$$746\,(1 + x) - 5480 = \tfrac{3}{2}\,[6450 - 680\,(1 + x)]$$

giving $x = 7\frac{1}{2}$ approximately.

This would mean that there are seven or eight immunizations to every clinical case notified in St. Pancras, and this certainly agrees with the estimate made by Frost [16] from Schick tests, that in the ordinary urban environment of Baltimore there were seven latent cases to each clinical case. I think, however, that this is an overestimate of x, for the assumption that *all* the children born into the population are going to be exposed to diphtheria infection, even in London, is not, I think, justified. For one thing, it leads to the conclusion that in November, 1926, only about 2% of all the children under 10 in St. Pancras were susceptible, which is scarcely credible. It has been proved conclusively that many children brought up in middle class homes remain Schick-positive, and most of these have presumably escaped exposure to the bacillus in any considerable doses. Let us suppose that in St. Pancras one-third of the families are sheltered from exposure to diphtheria and that one-third of the births are into such families; then if M' be the total susceptibles in the exposed families only on November 1, 1926, we get : $746\,(1 + x) - 3653 = M'$, and $4300 - 680\,(1 + x) = M'$ leading to $x = 4{\cdot}7$ and $M' = 410$; and, if we add to M' all the susceptibilities in the sheltered families, this means about 35% of the whole child population under 10 susceptible, which is not unreasonable.

A comparison of two curves by Collins [13] as representing the increase of Schick-negative children with age on the one hand and the proportion who had suffered from clinical diphtheria on the other in a very large mixed population in the United States of America shows 56 per cent. immunized and 9 per cent. who had had diphtheria by the fifteenth year, suggesting that $x = 5$, which agrees with the estimate of $4{\cdot}7$ above, and Dudley [3c] found a still lower ratio $x = 3$ for boys in

Greenwich Naval School, which he thought was to be expected owing to the exceptionally high infection pressure under dormitory conditions. Another point to be considered is that x may vary within certain limits even when communal immunity and other conditions are constant.

Turning now to *scarlet fever*, the incidence curve in fig. 1 shows an autumn outbreak in 1925, low incidence and no epidemic in 1926, a gradual rise of incidence in 1927 and autumn outbreaks in 1928 and 1929. Immediately preceding each of the autumn epidemics of 1925, 1928 and 1929, the index of apparent infectiousness rose suddenly to about twice its value in the post-epidemic periods. The period chosen as unit for computing the index was March 2 to August 1, 1925, when 119 cases occurred, followed by five secondary cases as defined in the same houses, a ratio of 4·20 per cent.[7] During the 1925 epidemic August 3 to October 25 there were 152 cases, followed by fourteen secondary cases, and during the 1928 epidemic September 3 to October 14 there were ninety-nine cases, followed by eight secondary cases, giving percentage ratios 9·21 and 8·08, or, if we combine these two, 8·76, which differs from the ratio in the period of lowest incidence December 26, 1928, to September 15, 1929, by $6{\cdot}15 \pm 1{\cdot}35$. We may therefore regard these rises as significant, and they doubtless indicate a sudden increase of true infectiousness due to a local upset of balance between the infection pressure factors and the immunity factors which was sufficient to start off an epidemic, though communal immunity was sufficiently high to prevent this going very far. Apparent infectiousness remained practically constant during the 1½ years from November 1, 1926, to May 1, 1928, and in this period there were 723 cases. Since we have definite evidence that latent immunization occurs in scarlet fever, and also that a considerable proportion of children retain an immunity from birth which must be inherent, let us again suppose x latent cases to each clinical case, and that a proportion y of children retain an immunity after the first year of age. The replenishment of susceptibles by 4,902 survivors of the first year of age during the period gives an addition of 4,902 $(1-y)$, so that if all families are at risk from infection by scarlet fever,[8] and there is no change in true infectiousness, we have $723\ (1+x) = 4902\ (1-y)$ giving $(1+x) = 6{\cdot}8\ (1-y)$. Zingher's results from Dick tests on 7,700 children in New York [17] showed that about 28 per cent. appear to retain an inherent immunity at the end of the first year, giving $y = {\cdot}28$, and that by the fifteenth year another 52 per cent. had become Dick-negative. Collins' analysis of histories in American town populations [13] shows that by the fifteenth year 11 per cent. have suffered an attack of scarlet fever, which suggests in conjunction with Zingher's result that $x = (52-11)/11 = 3{\cdot}7$, and in that case $(1+x) = 4{\cdot}7$ and $(1-y) = {\cdot}72$, giving the equation $(1+x) = 6{\cdot}6\ (1-y)$ which is in remarkably close agreement with my own equation, derived solely from the index of infectiousness.

It may seem to some who have a distrust of statisticians that the agreement of these estimates for diphtheria and scarlet fever with those of workers using other methods is too good to be true and that the figures have been manipulated to produce agreement. I can only assure you that this is not so, and the reasonableness of the results thus obtained caused me some surprise, for the material and range of years so far analysed are obviously very inadequate. The analysis for these two diseases was undertaken to illustrate the possibilities of the method and in the hope of establishing some confidence in it by checking it against other data, before we proceed to those other diseases where no such data exist.

Probably the progressive decline in scarlet fever incidence over the last centuries has been accompanied by, or caused by, a progressive increase in y, the communal

7 A better period would have been the first eight months of 1929, with an apparent infectiousness of only 2·61 per cent., but the data for 1929 were not available when the diagram was drawn; it would merely alter the scale of the index.

8 This must be nearly true in towns since Dick tests show 80 or 90 per cent. immune by adult life is reached.

inherent immunity, and also in x, which measures the capacity of the population rapidly to acquire immunity without severe reaction when exposed to infection, a capacity which may also be inherited.

Whooping-cough.—Nearly thirty years ago Laing and Matthew Hay [18] made an analysis of over 20,000 notified cases of whooping-cough in Aberdeen from 1882-1900 and pointed out that in that city epidemics tended to be roughly biennial, whilst in Edinburgh there was more often a three-year interval, in Glasgow a three- or four-year interval, and in Dundee a four-year period. They also thought that high-peaked epidemics tended to be followed by longer periods of quiescence than lower-peaked epidemics.

Thanks to the kindness of the Medical Officers of Health of Wandsworth (Dr. F. G. Caley), and of Holborn (Dr. C. W. Hutt), I have been able to make some observations from the notification records of these boroughs during recent years. The Wandsworth data covered the three years 1926-28, during which time 4,204 cases were notified, and the mean weekly incidence and index of apparent infectiousness are shown in fig. 2.

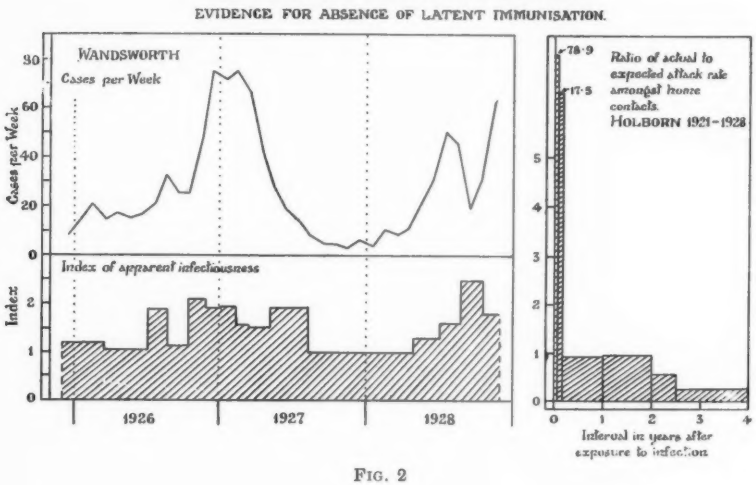


FIG. 2

During the first half of 1926 incidence was low, with about fourteen cases per week; from July 13 to August 23 it rose to thirty-three per week, fell again to twenty for the next eight weeks, and then followed an epidemic which lasted until the end of April with sixty to ninety cases per week occurring from November to March. Incidence was at a low level for fourteen months from May, 1927, to June, 1928, rose to about fifty per week during July and August, fell again temporarily during September, and reached epidemic proportions again at the end of the year.

For the purpose of calculating the index of apparent infectiousness, a secondary case was defined as a case under 15 years of age who began to develop whooping-cough four to twenty-eight days after another case of any age in the same house. Onset in whooping-cough is not of course very easy to date exactly, and the records in many cases only placed it to the nearest week, giving an irregular distribution of intervals, which has been smoothed in Table I.

It is evident from fig. 2 that the apparent infectiousness rose at the start of the 1926 epidemic to about double its pre-epidemic level, and remained high until the end of July, 1927, some two or three months after the epidemic had subsided. It then remained low for nine months and rapidly rose again to two and a half times its normal level before the autumn epidemic in 1928. During the unit period from August 9, 1927, to April 30, 1928, there were 266 cases, followed by 36 secondary cases as defined, a ratio of 13·6 per cent., whilst during the six weeks October 19 to November 29, 1926, there were 235 cases followed by 69 secondary cases, a ratio of 29·4 per cent., giving an index of 2·16 for this period. The sudden fluctuations shown in the diagram can safely be regarded as statistically significant and not due to sampling errors; thus in the six weeks July 13 to August 23, 1926, the percentage of secondary cases was 24·4 ± 2·0, in the sixteen weeks preceding it was 14·8 ± 1·6, and in the eight weeks following, 16·0 ± 1·9. These sudden fluctuations can scarcely be due to changes in communal immunity and must therefore presumably be due to changes in facility of transmission.

The absence of any gradual fall in the index during the progress of the epidemic, or of any pronounced difference between the indices before and after the epidemic suggests that latent immunization must be absent or at least not a factor of importance. This has been further tested from the Holborn notification records. Every child under ten who was a contact in the same house as a case of whooping-cough during the seven years from mid 1921 to mid 1928, and who had not had the disease, was followed up through the records and the date of any subsequent attack noted, and the interval since exposure as a contact. The age distributions of the total children at risk after the lapse of 6 months, 1 year, 1½ years, and so on, from the time of exposure were worked out and the expected attacks during intervals 0-½ year, ½-1 year, 1-1½ years . . . were calculated from these by multiplying the number at each age by the mean attack-rate per 1,000 susceptibles in the population at that age and making a correction for removals during the interval. The mean attack rates used for standardizing were obtained from the yearly births, deducting deaths and notified cases and thus arriving at the mean population of susceptibles at each age, giving, when divided into the notified cases, the following rates per thousand susceptibles during the septennium :—

Age	0	1	2	3	4	5	6	7	8	9
Rate per 1000	30·6	34·4	38·9	46·6	40·6	39·5	30·1	11·8	3·1	3·2

Thus there were 697 children under ten at risk over at least 1½ years, and the expected attacks on the basis of their age distributions and the above rates were 21·96 during a whole year or 10·98 during the half-year period 6-12 months after exposure, or after deducting the number likely to have removed after that time as calculated from the annual deductions from electoral registers, the expected attacks notified became 8·86. Proceeding on this basis for successive intervals after

TABLE II.—SUBSEQUENT BEHAVIOUR OF 721 CHILDREN UNDER 10 YEARS OF AGE WHO WERE CONTACTS TO WHOOPING-COUGH IN HOLBORN DURING 1921-28.

Time after exposure	Expected attacks	Actual attacks	Ratio of actual to expected
4-28 days	1·88	153	81·4
1-2 months	1·94	34	17·5
2-3 ,,	1·94	2	
3-6 ,,	5·82	7	0·93
6-12 ,,	8·86	6	
1-1½ years	7·75	7	
1½-2 ,,	5·80	6	0·96
2-2½ ,,	5·13	3	0·58
2½-3 ,,	3·61	0	
3-3½ ,,	2·96	0	
3½-4 ,,	1·91	1	
4-4½ ,,	1·46	1	0·26
4½-5 ,,	0·85	1	
5-5½ ,,	0·55	0	

exposure to infection the observed and expected attacks and the ratio between them were as shown in Table II. The ratios are also shown diagrammatically in fig. 2, and it is clear that, after the immediate risk of infection had subsided, the contacts were subject during the first and second year to almost exactly the expected rates and there is therefore no evidence of any acquired protection through exposure. The falling rates after two years, even if statistically significant, which they are not, could not be due to latent immunity.

The conclusion is that there is here twofold evidence against the existence of latent immunization to whooping-cough being acquired by close contact, but, nevertheless, in spite of little isolation of cases being practised at home and the length of the infectious period, it appears that out of 721 contacts under ten who had never had whooping-cough only 187 contracted the disease within two months and 221 within four years[9] of exposure. This suggests that about 70% of contacts under ten years of age who have not had whooping-cough will escape it altogether. In Wandsworth, the average cases per year from 1926-28 were 1,401, whilst the population of children was being replenished at the mean annual rate of 4,527 survivors of the first year, so it would appear that about 70 per cent. of all children born are likely to escape the disease. From the age distribution of cases it follows that about 80 per cent. of all children under ten who have not yet had the disease in the population will escape it altogether, as compared with about 70 per cent. of those who were contacts. It must therefore be concluded that the bulk of children escape whooping-cough by virtue of an inherent immunity rather than by avoiding contact with infection.

Chickenpox.—Chickenpox offers in this country an almost virgin soil to the statistician, apart from some hospital statistics, but fortunately it has been notifiable in Paddington for a number of years, and thanks to the Medical Officer of Health, Dr. G. E. Oates, I have been able to work out the index of apparent infectiousness over five years from 1925-29. Fig. 3 shows the mean weekly case incidence in each month during the seven years 1923-29, and the resulting curve presents some interesting features. In each of the seven years there was a peak either in June or July, and the heights of these successive summer peaks form such a beautiful series that I could not resist the temptation to draw a spline curve through them as shown by the dotted line. Whether or not the periodicity of about six years which this suggests is a real one and not merely accidental, I cannot say with confidence, and I am afraid all we can do is to wait and see.[10] The peak month was June in 1924, 1925, 1926, whilst the annual incidence was increasing, and July whilst it was decreasing. There was also in each year, except 1929, a second peak in October or November, and one might also draw a curve through the tops of these which would be roughly inverse of the other, that is to say a small summer peak was usually followed by a considerable autumn peak. The annual totals of cases from 1923 onwards were successively 753, 718, 852, 1,123, 788, 924, 558.

The distribution of intervals between cases which occurred in the same house is shown in Table I, and a secondary case for the purpose of the index of apparent infectiousness was defined as a child under 15 who developed the rash twelve to sixteen days after a previous case in the same house. The changes in the index during 1925-29 are shown in fig. 3, and it may first be noticed that every one of the nine epidemic periods, whether summer or autumn, was preceded by a sudden increase in apparent infectiousness to three or four times its ordinary level. To prove that these fluctuations are statistically significant we may take, for example, the first one, in 1925: thus, from January 1 to April 4 there were 160 cases, followed by twenty-five secondary cases, a percentage ratio of $15 \cdot 6 \pm 1 \cdot 9$, and from

9 The mean period at risk of the 721 cases was 4·1 years.

10 It is interesting to notice that the annual totals of reported cases in Chicago from 1911 to 1926 [19] show minimal levels in 1912, 1918 and 1925, and maxima at 1917 and 1924.

April 5 to May 2 there were sixty-five cases, followed by twenty-four, a percentage of 36·9 ± 40, the difference being 21·3 ± 4·5. These sudden fluctuations must be due to changes in true infectiousness.

In 1926, when the highest summer peak was reached, the sudden access of apparent infectiousness occurred in March; in 1925 and 1927, when a moderate summer peak was reached, the rise in index did not occur until April, and in 1928 and 1929, when there were only very small summer peaks indeed, it was delayed till May or June. Evidently infectiousness must rise early in the spring in order to produce a large summer epidemic. An invariable feature of the incidence curve is a low rate in September, and it might seem that an obvious explanation of the break between the summer and autumn maxima is the summer holiday, which stops the spread of infection in schools, and that apart from this they would be fused into one. I think the behaviour of the index proves that this is not the real explanation. The index cannot be appreciably affected by school closure, for the

CHANGES IN APPARENT INFECTIOUSNESS OF GERMAN MEASLES AND CHICKEN POX. 1923–1929.

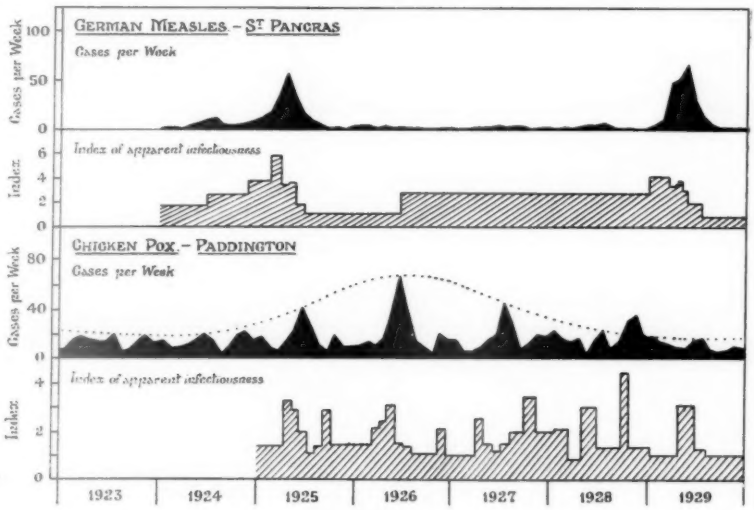


FIG. 3

spread of infection which it measures is between children living together in the same house, and since contacts are excluded from school, the children will be as much together at home whether schools are closed or not. It appears from the data that in every year the infectiousness gradually or rapidly subsided from a high to a low level *early* in July, and did not rise again to a high level until October or November, proving, I think, that the low incidence in August and September was the result of seasonal factors unconnected with school closure.

If we seek to relate the rise and fall of the index from one steady period to another, with the depletion of susceptibles occurring during the interval, then, measuring the index at the middle of steady periods of low incidence, we find that from January, 1925, to February, 1926, the index rose from 1·39 to 1·42, with 988 cases; from February, 1926, to March, 1927, it fell to 1·03, with 1,153 cases; from March, 1927, to April, 1928, it fell further to 0·78, with 1,064 cases; and from April,

1928, to March, 1929, it rose again to 1·01, with 822 cases during the interval; thus :—

No. of cases ...	...	822	...	988	...	1,064	...	1,153
Change in index	...	+·23	...	+·03	...	−·25	...	−·39
Interval in months	...	13	...	13	...	13	...	11

This suggests that the apparent infectiousness was maintained at a constant level (apart from seasonal fluctuations) when cases were occurring at the rate of about 950 to 1,000 in thirteen months, or 900 per annum. The annual births in Paddington during 1922-28 added fresh children to the population at an average rate of 2,223 survivors of the first year of life per annum, so it appears that to every two notified cases there are three children who either have inherent immunity or have chickenpox unnoticed or unnotified, or acquire latent immunity to it.

In order to ascertain whether latent immunization does occur, a method is now being applied which has been made possible through the courtesy of the Health and Education departments of the London County Council, but I have not yet been able to follow up enough contacts to answer the question with certainty. I give the preliminary figures, postponing any elaborate analysis until much larger data have been collected for this and the other diseases. By following up through the records of the various departments 151 children, aged 5 to 7, who were excluded from eight Paddington schools during 1926-28 as contacts to chickenpox cases at home, it was found that twenty-seven developed the disease within three months, or about 18 per cent., leaving 124 at risk at the end of three months. These were followed further through the records over varying periods up to three years, during which none passed the ninth year of age. The aggregate number of years at risk was 219 and four cases of chickenpox occurred, or 1·8 cases per 100 years at risk. The control group, selected in exactly the same way at the same ages[11], in the same years and at the same schools, except that they were excluded as contacts to some disease other than chickenpox, consisted of 296 of whom three contracted chickenpox soon after exclusion, leaving 293 at risk after the lapse of three months. These were then exposed to risk over an aggregate of 461 years, and twenty-three cases of chicken pox occurred or 5·0 cases per 100 years at risk. The probable errors of these rates are of the order of ·7 and ·8 respectively, so the difference between 1·8 and 5·0 may be regarded as statistically significant. The rate of five cases per 100 years at risk implies a mean annual attack-rate at ages 6, 7 and 8 combined[12] of about 5 per cent., and this may be compared with the mean attack-rates per 100 in the whole population of Paddington at these ages during 1925-28, which were approximately 7·1, 4·4 and 2·5 respectively, or a mean rate of 4·7.

The lower attack-rate of 1·8 per cent. in the children who had been exposed to infection but escaped, might conceivably be due to either (1) selection by the disease of the most susceptible, leaving a residue less susceptible on the average than the controls, or (2) latent immunization of those exposed.

As regards selection the facts might be explained if we suppose that about half the children born have an inherent immunity to chickenpox and escape the disease for that reason.[13] Another quarter will have chickenpox by 6 years of age[14], leaving about a quarter of any random sample about that age susceptible. On this basis of 100 contacts about this age eighteen catch the disease within three months, leaving eighty-two at risk, of whom only seven are susceptible, whereas of 100 controls only one catches it within three months, leaving ninety-nine at risk, of

11 The mean age of the control group was 5·88, and of the contacts 5·75 years, the percentages aged 5 being 36·6 and 37·8 respectively.

12 The 5-year-olds at time of exclusion would average 5½ years, and after allowing 3 months to elapse, 5¾ years, before they were at risk.

13 The histories analysed by Collins show that 52% reach the age of 17 without having chickenpox in American towns, and the Paddington data suggest a similar figure.

14 The Paddington data for 1925-28 show that of 3,696 cases 2,027 occurred before age 6.

whom twenty-four are susceptible, so that the subsequent attack-rates, given equal chances of infection, should be as 7/82 to 24/99, or as 1·8 to 5, which is the ratio actually found.

On the other hand these facts could be equally well explained by latent immunization with about one latent case to each clinical case, and to settle which of these is the correct explanation or whether both play a part, further data are necessary. I have worked out the attack-rates up to the end of 1928 amongst 3,416 contacts at different ages up to 10 in the whole of Paddington during 1925-28, and the results are shown in Table III. After the first year, when there is as usual evidence of some congenital immunity, there is a steady fall in attack-rate among

TABLE III.—FATE OF CONTACTS TO CHICKENPOX IN PADDINGTON, 1925-28.

Age	Total	Per cent. of contacts who had not previously had chickenpox	Per cent. of these who contracted it up to the end of 1928
Under 1 year	215	99·5	43·0
1-2	515	98·6	58·6
3	373	92·0	55·1
4-5	693	75·7	50·5
6-7	687	62·4	42·2
8	316	47·8	36·4
9-10	617	41·5	30·9

contacts from about 60 per cent. in the second year of life to about 30 per cent. in the tenth year, and this suggests that there is a process of latent immunization of a fairly permanent character in progress, for if there is only an inherent immunity the rate should not vary much with age. The combined evidence so far as it goes suggests that a certain proportion, possibly about a quarter to a third, of children born retain an inherent immunity, and that latent immunization protects others, possibly another quarter, but if this is so the ratio x of latent to clinical cases must be low, perhaps one to every two cases. Further research on these lines will doubtless settle these points.

Measles.—As early as 1913 Butler [12], in a very interesting paper to this Section, suggested the possibility of latent immunization in measles, whilst Halliday [20] in his admirable report on measles in Glasgow tenements, published a few months before my first paper on measles was in the press, actually made the suggestion that there might be a latent immunization of a temporary character. I confess that whilst I had read both these papers I had overlooked the sentences in which these suggestions were made. In a second paper on measles recently published in the *Lancet* [21], a graphic comparison of the St. Pancras data with the Willesden data of Butler has been made, and I have also carried the analysis of a measles epidemic a stage further and produced some additional evidence for a temporary latent immunization of the child population during an epidemic. Only brief reference need be made here to measles, and I have reproduced fig. 4 in order to show how the behaviour of the index of apparent infectiousness compares with its behaviour in the other diseases. The distinctive feature is of course the fairly rapid rise of index between one epidemic and the next, which has been shown to be far too rapid to be accounted for by the mere accession of fresh children at risk to the population. On this is superimposed the usual sudden increase in true infectiousness which appears to remain enhanced throughout the epidemic when a large area is being dealt with as here, but when analysis is made into smaller districts about half a mile square, it is found that as the epidemic travels slowly from one square to another, it is always preceded by a sudden access of infectiousness to about ten to fifteen times its post-epidemic level, which then steadily subsides in about six weeks, and thus the composite picture makes it seem that the infectiousness is at a high level throughout.

When the progress of the epidemic was thus followed over the map it was found to follow streams of flow depending upon barriers to free communication produced by extensive railway depôts and it appeared that the index of infectiousness rose and the epidemic started, for example, some twelve weeks later in Chalk Farm area than in Tufnell Park a mile away. These facts may later throw an interesting light upon the puzzling results which Brownlee obtained from periodogram analysis, but they seem at the moment to prove that the explanation which he gave of those results cannot be a correct one. They also show that in producing the sudden access of true infectiousness which precedes the epidemic wave wherever it travels, atmospheric influences must be of secondary importance, and the probable explanation is on the lines of Gill's quantum theory, that when the balance between the number of cases or carriers and atmospheric conditions on the one hand, and the density, freedom of circulation in schools and streets and average immunization of the

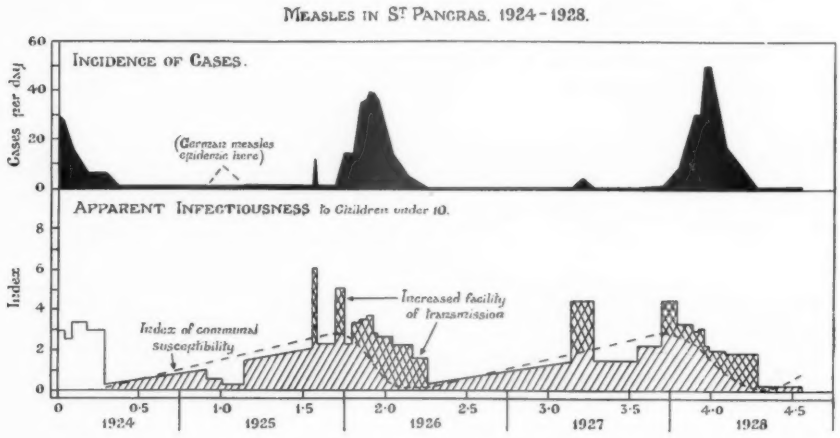


Fig. 4.

population on the other, passes a critical point, transmission of the virus is stimulated, and the epidemic goes forward till all the children who are not specially sheltered have been either permanently or temporarily immunized.

In fig. 5 I have shown what must happen to the child population in an urban area when subjected to a measles epidemic on the assumptions, all supported by a certain amount of evidence: (i) that an epidemic occurs every two years, with three-quarters of the biennial total of cases occurring in the epidemic six months and one-quarter in the eighteen months between; (ii) that children fell victims to measles in successive two-year periods at the rates obtained by Collins [13]; (iii) that latent immunes are produced at a rate of three to each case; and (iv) that latent immunity is lost at the rate of 50 per cent. in one year and 100 per cent. in two years. It appears that on this basis almost complete immunization of the mobile portion of the child population (aged three and upwards) will be brought about before the epidemic ends, leaving only very young children unprotected, doubtless those not in families with older children and who have therefore avoided contact with sources of infection.

A fuller explanation of this analysis and that of the progress of an epidemic in space is to be found in the paper referred to [21].

German measles.—In St. Pancras German measles is a notifiable disease, and during the six years 1914-29 there were 2,484 cases, of which 1,940 occurred in the two epidemic years 1925 and 1929. In the long periods between epidemics the incidence was at the rate of one to five cases per week, and, at the height of an epidemic, sixty to seventy per week. The distribution of intervals between successive cases in the same house, during 1924-28, is shown in Table I, and the most suitable range of intervals for the purpose of defining a secondary case seemed to be six to twenty-two days. The incubation period is more variable than in measles, and smaller intervals of days than six were probably for the most part due to infection from a common source, and cases occurring after such short intervals may be termed associated primary cases. It makes little difference whether the index be calculated on the basis of the secondary cases alone, or whether the associated primary cases are also included, since both tend to rise or fall with infectiousness; hence the numbers being somewhat small for this disease, the index was calculated on the basis of any interval up to twenty-two days, the second case being a child under 15, and the date of onset being the date of appearance of the rash.

IMMUNITY CHANGES IN THE CHILD POPULATION OF AN URBAN AREA DURING A MEASLES EPIDEMIC ON THE THEORY OF TEMPORARY LATENT IMMUNISATION.

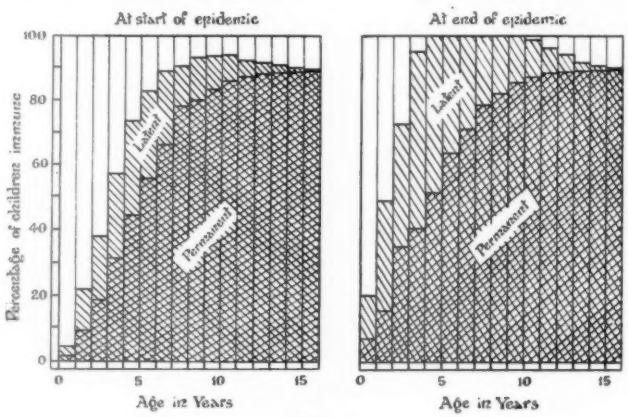


FIG. 5.

The behaviour of the index throughout the six years and the curve of cases per week are shown in fig. 3, and we again find the gradual rise in apparent infectiousness between one epidemic and the next, the sudden increase just preceding the epidemic, and the rapid fall to a low level whilst the epidemic is in progress. The rate at which the index rose between the epidemics averaged a 2·8-fold increase in one and three-quarter years, from January, 1926, to October, 1927, and from this we may conclude that the susceptible children under 15 doubled in numbers in a little over a year. During this period cases of all ages were being notified at the rate of 111 per year, and the population replenished at the annual rate of 3,225 survivors of the first year of life, so that about 3,100 were being added per annum to the population at risk, or between one epidemic and the next about 12,000. Since each epidemic consisted of only about 800 notified cases, we must either assume that there is widespread inherent immunity to German measles, or else that latent immunization is an important factor, as in measles, and which of these is the correct explanation can only be settled by further research.

In conclusion, I should like to urge that everything possible should be done to make our notification records as accurate as possible, for important advances in our knowledge of the mechanism of epidemics can undoubtedly be made from a study of them, and that local authorities may be requested not to destroy their notification forms as is now the usual practice after three years, thus rendering research of this kind almost impossible, but to keep such records, so that in future years they may be available for research. On this latter point I speak with feeling, for owing to the destruction of records it will now be necessary to wait five or ten years to settle some simple and fundamental points which could have been settled now if they had been kept. And finally, I would urge that a few more local authorities may use their powers to introduce compulsory notification of such diseases as chicken-pox, whooping-cough, measles, and influenza, in order to make statistical research possible.

I wish here to record my indebtedness to my assistant, Miss M. N. Karn, for her help in collecting and analysing the data, to Miss I. MacLearn, for drawing the diagrams, and to the Health Departments of Paddington, Holborn, Wandsworth, St. Pancras, and the London County Council, for their assistance.

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Discussion.—Sir WILLIAM HAMER: Forty years ago experiences recorded by W. H. Power at Brailes and Pirbright compelled school doctors to realize the importance of the "missed case." This lesson was forced upon my attention by an outbreak of diphtheria at Lewisham Bridge School in 1896, and, here in this room seventeen years later, a paper was read, in which it was stated that I must have suspected the existence of "bacteriological carriers," "missed" and "unrecognized" cases, for I "had referred to such cases under the generic name of throat illness." I blushed, needless to say, on hearing my true character thus revealed, as originator of the "healthy bacillus-carrier" hypothesis, but felt some doubt and misgiving on the subject, inasmuch as one of the main points stressed in my report was the discovery of a number of "mild *cases of illness*, the nature of which had not been recognized." My rough guess, in the Milroy Lectures of 1906, was that London measles-susceptibles ranged (at that time) between 180,000 and 120,000, and this guess tallies fairly well with Mr. Soper's recent and precise statistical analysis for London as a whole. Of course, studies made in Plumstead by Dr. C. J. Thomas and Dr. Sidney Davies, a quarter of a century ago, would yield a very different range, did we but (to quote Sir Thomas Browne's phrase) "difference nearer and draw into a lesser circle" of the infants' departments of the elementary schools of poor neighbourhoods: for Dr. Thomas writes that "the second crop exhausts practically all the susceptible cases." Moreover, one London prevalence may be particularly severe in the south-east, another in the East End, and so forth; and thus the Plumstead experiences of Dr. Thomas and Dr. Davies stand shoulder to shoulder with those of Dr. Percy Stocks in Chalk Farm and Tufnell Park. With such facts in mind, the number of susceptibles in infants' departments of hard-hit poorer quarters of London might well show reduction, during a measles outbreak, from, say 25,000 to 5,000, a range quite wide enough to justify the conclusion that "rise and fall in the proportion of

children protected by previous attack of measles" in such infants' departments, might well "be regarded as the chief cause of periodicity." As showing that, even nowadays, many cases of measles are "missed," or "unrecognized," I prepared a table (*Ann. Rep.*, 1913, Diagram H, opposite p. 199) which reveals the fact that a quite noticeable modification of the graph representing school notifications of measles is that produced during the period immediately after Easter, when "scheduling" is in progress. The depression is "not so deep as a well nor so wide as a church door," but "'twill serve" to prove how marked an influence is exerted upon notification statistics by any relaxation in the extent to which "following-up" inquiries regarding absentees from school are being pressed. Such considerations as these may be mentioned as supporting the recommendation (made by Dr. Stocks) that "everything possible should be done to render our notification statistics as accurate as possible."

Professor M. GREENWOOD: The researches upon which Dr. Stocks is engaged, part of which has just been described, seem to me among the most important contributions to epidemiological science now being made. Dr. Stocks, like Professor Topley and myself, is not proposing for himself so noble a quarry as Sir William Hamer and Dr. Crookshank, or the late Dr. Brownlee had, if not in view, at least in mind, viz., a general doctrine of epidemicity. He is seeking to describe accurately the particular phenomena of particular illnesses, using what I may call a general demographic method. In his work the "exposed to risk"—as an actuary would call them—are made up of smaller groups—e.g., family groups of different sizes and under different environmental conditions—for each of which the conditions are, perhaps, different, and he is seeking measures of the *average* effects. This is a fundamentally important line of approach, and Dr. Stocks deserves great credit for perceiving that notification data afford material and for *acting* upon that perception. I do not quite share his belief that a general extension of the system of compulsory notification, either geographically or with respect to other diseases, would be helpful. One or two areas, the public health officers *and* the clinicians of which have been convinced of the interest of the work and have agreed on criteria of notification, will provide far better data than ten times as many districts in which a system has been adopted which they regard with indifference, or even dislike.

This general demographic method is also used by Professor Topley and myself in our studies of mouse-villages, and in some respects our results run parallel with those of Dr. Stocks, as do our difficulties of interpretation. We, for instance, are satisfied that acquired immunity, an immunity which is certainly far from absolute and probably transitory, is an important factor and we too have experienced Dr. Stocks' difficulty in assessing the respective shares of such acquired immunity and of natural resistance.

Side by side of these general demographic studies, intensive studies are being made: Surgeon Commander Dudley's work is an important example. He is able to watch the happenings in a small community, the vital statistics of which he knows accurately. Similar, but of course less exact, work is being done in various public schools under the general guidance of a research committee appointed by the Medical Research Council.

I have also been interested for many years in the analysis of the distribution of multiple cases in houses containing different numbers of exposed to risk. During the War, Mr. Udny Yule and I were led to study a cognate problem, viz., the frequency distribution of accidents sustained by factory workers. We wished to discover whether the form of frequency distribution—number of exposed who had had no accident, number who had had one accident, number who had had two accidents, etc.—would enable us to form some judgment as to ætiology, i.e., whether accidents were really "chance" events, whether sustaining one accident made the victim more (or less) susceptible to a second accident, etc. We obtained and published ten years ago a first approximation to the solution of this problem. The problem of cases of disease is somewhat different, thus, while there is no theoretical limit to the number of accidents an individual might sustain when the period of observation is long, there cannot be more than n "cases" of disease in a household of n members. There is, however, close similarity between the statistical methods applicable. Sufficient work has already been done to make it certain that the distribution of multiple cases of such a disease as measles is neither random, in the statistical sense of the word, nor does it approximate to the opposite extreme, viz., that when there are n exposed to risk, the frequency will collapse into the mere discovery that all of the n will take the disease. Before we can usefully devise a mathematical schema, we must set our scientific imaginations to work and speculate about what really happens in a community wherein n susceptibles are exposed to, say, one infective

person. Is there a process of bombardments, does one "hit" in each of successive time intervals lead to a different clinical result from two "hits" in the same interval, etc.? A very little reflection shows how difficult and how fascinating this sort of investigation is and also that frequent cross-references between the demographic and the intensive researches are imperative.

Surgeon Captain S. F. DUDLEY said that one difficulty alleged by some workers in accepting any latent immunization hypothesis was that carriers were not sufficiently ubiquitous. He thought this objection was due to a failure to realize that if the duration of infection was short, a large proportion of the community could become infected, although at any instant of time the carrier-rate was small. That this was true he had proved directly in the case of diphtheria by swabbing a random sample of 139 normal schoolboys eight times in the course of one year. Fifty-five, or 40%, of this sample were found to carry Klebs-Loeffler bacillus (about half toxigenic) on one or more occasions during the year, but only 9% were infective at more than one of the eight swabbings. The percentage of total swabs infected was 6; about the same carrier rate as in the L.C.C. schools. If the same set of conditions continued over the ten years of school life, there would be enough different latent infections for every school child to be a carrier four times. He was glad Dr. Stocks was giving attention to the "circulation factor" or the effect of the amount and type of motion of individuals within, without, and to and from the herd. It was a factor that must be vary variable at different times and places, yet it must be of great importance but presenting very great difficulties in measurement. Finally, as an amateur biologist, he was loth to give up his belief that some kind of adaptive variations in the characters of infecting agents, such as parasitic "herd virulence," was not just as essential a cog in the mechanism of herd-infections as herd-immunity.

Dr. J. D. ROLLESTON, after alluding to the "formes frustes" of scarlet fever described by Trousseau, and the "febris morbillosa" of Sydenham, as examples of incomplete forms of infectious disease in the pre-bacteriological era, said that the staff of a fever hospital offered an excellent opportunity for the study of latent spontaneous immunization. Infectious diseases were most likely to attack the young nurses or medical officers who had recently joined the staff, but, occasionally, as Zoeller[1] had shown, the action of spontaneous immunization was capricious, and persons who for many years had been in contact with infectious cases suddenly contracted diphtheria or some other acute disease to which it might have been supposed they had become permanently immune. According to Zoeller, no less than 36 per cent. of the medical staff in departments for infectious disease showed a positive Schick reaction and therefore required active immunization.

Dr. STOCKS (in reply) said that the possibility of variation in the normal level of virulence of the infecting agent owing to "biological modification" must not of course be lost sight of, but if we were to arrive anywhere in research of this kind we must assume some factors to be constant. A step forward had been made in attempting to take into account the simultaneous variation of two factors instead of one only, and at present it was necessary to suppose that the same strain of virus persisted in a district over the short period of years under review. In regard to Sir William Hamer's difficulty as to how a ratio of, say, three latent cases to one clinical case in measles, fitted in with the fact that in an infants' department of a school heavily attacked by an epidemic, almost every child at risk sometimes took the disease—this was doubtless a question of intensity of infection. If a large number of cases or carriers happened to occur together in a school, the velocity of infection would be such that most children at risk would succumb to a clinical attack, and the ratio in that school would be less than three to one, whereas if cases occurred at intervals, so that smaller doses of infection had to be dealt with in a given time, many of the children would acquire latent immunity rather than a clinical attack and the ratio would be greater than three to one. The ratio calculated in St. Pancras must be regarded as an average ratio over the whole district.

1 *Bull. et Mém. Soc. méd. Hóp. de Paris*, 1928, lii, 426

Section of Tropical Diseases and Parasitology.

[*February* 6, 1930.]

Bilharziosis of the Blood-vessels.

By M. F. Sorour

(*Assistant Professor in the Department of Pathology, University of Egypt, Cairo*).

The still vague ætiology of diseases of the blood-vessels justifies the publication of these observations on lesions produced by the bilharzia worm and its ova in these vessels. Bilharzia worms are found in the blood collected from the main trunk of the portal vein, the opening of which is a routine procedure in our post-mortem technique. The number of worms here varies with the severity of the infection, but not with the severity of the lesions, as the worms might have crept down into the portal radicles and the pelvic veins, producing severe lesions at their destinations, leaving only a few in the main portal vein. Worms have been found in the splenic vein, but only in small numbers and in a small percentage of cases.

In certain cases we have found the intrahepatic portal branches packed full with bilharzia worms which looked young and immature, while the main trunk of the vein outside the liver may contain none or very few. Both sexes of the worm are found: in some cases, only males are met with in the portal vein, but as in these cases we always found bilharzial lesions, the females must be present in the finer branches, where they are found in microscopical sections. Natural unisexual bilharzial infection must be very rare, although theoretically possible. Repeated examinations of the portal vein did not show any change in its walls, not even endothelial thickening. In microscopical sections of bilharzial lesions, especially in the large gut, we frequently see veins which are the seat of organized thrombosis, with re-canalization, and which in serial sections always show a bilharzia worm—or bilharzia worms—not far from the affected spot in the vessels. Again, if a bilharzia worm dies inside a vessel, proliferation of its endothelial lining takes place all round the lumen in a marked way, and this no doubt predisposes to thrombosis, while the potent new tissue-forming bilharzial toxins encourage the organization process, which is no doubt much denser in these bilharzial cases than in any other thrombus organization. The bilharzia ovum is also capable of producing most marked lesions in the walls of blood-vessels: lesions that are, indeed, exactly similar to the well-known vascular lesions found in syphilitic and arteriosclerotic cases with no bilharzial element.

Thus if a bilharzia ovum remains in a small blood-vessel, it excites proliferation of the endothelial lining all round the lumen which gradually narrows till it just holds the irritating ovum. This is a pure endothelial lesion. If, however, the ovum, although in the lumen of the vessel, is partly engaged with its spine in the intima, so as to touch the sub-endothelial tissue, the latter tissue proliferates, but only in the sector where the bilharzia ovum is engaged. The lesion thus produced is a pure sub-endothelial one and resembles exactly the local arteriosclerotic lesions found elsewhere. The bilharzia ova can and do get complete access to the sub-endothelial layer of blood-vessels, especially in the lungs, and this, I think, is because in the lungs the ova are laid by the mother worms in branches of the pulmonary artery, which have thicker walls and therefore wider vasa vasorum which let in the ova to the inner coats of the vessels. In the sub-endothelial tissues the bilharzia ova may exist in appreciable numbers, and their irritation produces great proliferation of that tissue, with giant cells and eosinophils. The lesion thus formed would be exactly similar to that of ordinary arteriosclerosis, were it not for the giant cells and eosinophils, which are constantly associated with bilharzial lesions elsewhere. Even degeneration and actual calcification occur in

these bilharzial atheromatous lesions, thus completing the picture of ordinary arteriosclerosis and atheroma.

If the ovum is caught in the muscular coat of the vessel, it produces a bilharzioma in the part of the wall where it is lodged. This bilharzioma, at first, consists of endothelioid cells but later on it is fibrosed by concentric layers of hyaline fibroid tissue, and as it swells out beyond the line of the vessel it looks much like a small saccular aneurysm that has healed and fibrosed. It will be seen, then, that the bilharzial factor produces vascular lesions exactly similar to those found in ordinary arterial diseases, which may also be due to irritating factors having gained access to the walls of the vessels and produced the proliferations and degenerative changes.

Apart from lesions produced by the worm or its ova, there are lesions produced in the blood-vessels by the toxins of bilharzia. Thus, in certain cases of ordinary bilharzial bladders, we have met with endotheliomatous growths in the wall of the bladder. The blood-vessels in these cases were seen to be much narrowed or completely obliterated by pure endothelial proliferation, while the tumour mass in the bladder wall consisted of groups and columns of endothelial cells separated from each other by fibrous tissue, rich in eosinophils only. We believe such endotheliomata to arise from the vascular endothelium which is irritated to excessive proliferation and actual tumour formation by the bilharzia toxins which are especially potent as endothelial irritators. This is quite a different picture from that of bilharzial epithelioma of the bladder in which the growth is sometimes found inside the lumens of the blood-vessels—a condition that has been described by my late teacher, Professor Ferguson.

[*March* 6, 1930.]

The Story of Leprosy at Nauru.

By GEORGE W. BRAY, M.B., Ch.M.

(*Late Medical Officer, Nauru, Central Pacific.*)

NAURU, or Pleasant Island, is an oval-shaped atoll in the Central Pacific Ocean, practically on longitude 167° East, twenty-six miles south of the equator. The island is about twelve miles in circumference and has a population of about 2,500, 1,200 being native Nauruans and of the remainder, just under 1,000 are indented Chinese labourers. From 1888 to 1914 it was a German possession, but is now a Mandated Territory, administered by the Commonwealth of Australia. Its main claim for notoriety lies in its very rich phosphate deposits, the mining rights of which are vested in the Governments of Australia, Great Britain and New Zealand. It is thus self-supporting, and the natives, grouped for administrative purposes into fourteen districts, each presided over by a chief, play the rôle of rich land-owners, receiving both rents for their land and royalties on each ton of phosphate shipped. In earlier years the actual mining was carried out by natives recruited from the neighbouring island groups such as the Carolines and New Guinea, but nowadays Chinese are the sole recruits.

The nearest neighbouring island is Ocean Island, the capital of the Gilbert and Ellice Colony, 160 miles distant. The Solomons lie 600 miles to the south-west, the Carolines 400 to the north-east, and the Gilberts 250 miles east.

It has been stated that "This study of leprosy has a special value by reason of the unique conditions. The native population is entirely isolated, having no connection with neighbouring island groups. The introduction of leprosy is a recent event, the disease being unknown before 1911, and the conditions are exceptionally

favourable for close supervision of every person on the island. The course of the disease will have an exceptional interest."

The aim of the Administration has been well summarized as follows: "The Administration views with extreme apprehension the presence of leprosy on this island, and has spared neither effort nor reasonable expense in a whole-hearted endeavour to attain the ideal, namely, the entire checking and elimination of the disease."

As the clinical aspect of the leprotic lesion is so well described in all text-books and the characteristics of the lepra bacillus so well known to all, I will omit these particulars and endeavour to discuss the remaining facts under three heads: (1) The story of the onset; (2) the story of the spread; (3) the story of the conquest.

(1) *The Story of the Onset.*—There appear to have been three possible sources of infection of the native Nauruan population:—

(*a*) Shortly before 1912 a Caroline islander came to Nauru, resided for an unknown period, was declared a leper and repatriated in a cage slung over the side of the vessel. He was in close contact with the natives and particularly with the first known Nauruan leper Demau.

(*b*) In 1912 a Gilbertese woman, Etsio, settled on Naura in close proximity to Demau. She formerly lived in Tahiti, then on Ocean Island, where she has a son in the leper station. Though pronounced leprous on her arrival at Nauru by the doctor, she was permitted to land by order of the German Governor. She settled in Ewa, close by Demau, who was then a young girl. She died on November 21, 1914, and was buried in Ewa.

(*c*) A Chinaman after eight years' residence on Nauru was found to be leprous on repatriation in 1920. From time to time sporadic cases occur amongst the Chinese, but if any signs of the disease are evident on arrival at the island, prompt repatriation results. These Chinese labourers are enclosed in wired locations and any association with the natives is strictly guarded against.

At Nauru, as among the other Pacific island groups, leprosy seems to be intimately associated with indented labour, particularly from China where leprosy is endemic. In 1925, in the Gilbert and Ellice groups (population about 31,000), there were twenty-eight known cases of leprosy. At Ocean Island, the capital, there were five cases, all but one of whom had served a period of indenture at Tahiti, the disease appearing 7-9 years after their return. Previously it was unknown. Again, in Western Samoa, a Mandated Territory administered by New Zealand, amongst a population of 35,000, fifteen cases had been discovered by 1925 and removed to Mokogai, Fiji, for treatment: Six Samoans, three half-castes, five Chinamen and one Rarotongan (thirteen males and two females in all).

(2) *The Story of the Spread.*—By June, 1920, eight years after the arrival of the possible sources of infection enumerated above, four cases of leprosy were under observation and the diagnosis proved subsequently bacteriologically:—

(1) Demau, who lived in Ewa in close proximity to Etsio and later moved to Anabar. She was a frequent visitor to her friends and relatives and stayed with them for varying periods.

(2) Moses, of Boe, a man of more reserved nature, who did not visit much.

(3) Robek, of Buada, a lad of 15 years.

(4) Eideraqui, a woman aged about 40, who spent some time in Menen, then in Yarren, then in Boe.

In October, 1920, four months later, an epidemic of influenza of the pneumonic type swept the island claiming a 100% incidence and a 30% mortality. Of these four known lepers Demau alone survived. Not only did the epidemic claim this vast mortality, but it left in its train an even greater morbidity; ill health and debility were everywhere rife, a condition of affairs only too readily augmented by a definite

dietary deficiency. To the prevalence of this state of greatly lowered resistance the rapid and acute spread of leprosy has been attributed.

That the rapidity of this spread may be readily appreciated, the yearly incidence of the disease from 1920 to 1928 is tabulated:—

December 31	Patients in segregation	Leper clinic out-patients	Total	Releases on parole
1920	4	—	4	—
1921	10	50	60	—
1922	139	103	242	—
1923	151	144	295	—
1924	193	153	346	21
1925	189	176	365	28
1926	167	169	336	35
1927	174	163	337	—
1928	132	86	218	46

Within four years of the influenza epidemic the spread had reached its summit and 30% of the entire native population were showing clinical manifestations of infection.

Amongst the first thirty-four definitely proved lepers, nine were relatives of Demau and three of Moses, and the most heavily infected districts were those where the four original patients had lived. One is therefore justified in surmising that all infection came from those four original cases, especially from Demau, who alone survived the influenza epidemic, and that Demau herself was in all probability infected from the Gilbertese woman Etsio by whom she had lived as a girl for nearly two years.

A few observations on the characteristics of the disease as manifested at Nauru may not be out of place at this juncture.

Of the three "text-book" types described, about 90% are of the maculo-anæsthetic variety, the few remaining cases being equally divided amongst the nodular and mixed types. The lesions occur mostly on the exposed parts, the arms, thighs, chest and abdomen. The primary lesion generally has a central scar, the evidence of a previous sore or boil, a fact which suggests but one possibility, i.e., local inoculation. Cases with an acute onset with fever and rash are rarely observed. Acid-fast bacilli have been almost entirely absent from the nasal mucus in all cases. Leprotic mutilations and deformities have rarely appeared.

Practically every native has been in contact with a leper, and nearly every family has at least one representative, with the exception of one district, Ijuw, in which no case has appeared.

The cases are fairly evenly distributed amongst the sexes. With regard to the age incidence, in 50% of cases the onset has been before the twentieth year. In 1926 the figures were as follows:—

Age	Percentage	Age	Percentage
1-10 years	14·2	30-40 years	18·3
10-20 ,,	37·3	40-50 ,,	7·1
20-30 ,,	20·1	over 50 ,,	3·0

Childhood and puberty are thus shown to be the most susceptible periods, and it is at these periods that the most stringent regulations to prevent infection must be enforced.

(3) *The Story of the Conquest.*—Early diagnosis is the essential factor. Each month the medical officer inspects the entire native population, called together by the chiefs and checked according to roll. Attendance is obligatory, and the greater part of the body surface is exposed. Any suspicious cases immediately attend the Government hospital for more detailed investigations, which include intensive clinical examination, nasal swabbing, clamping and pricking or sectioning of nodules, dissection of lesions—if small, total resection—and preparation of smears

and stained paraffin sections, and finally—the most important and accurate procedure—the dissection for, and the staining of sections of, subcutaneous nerves entering the lesion.

According as the cases are definite and infective they are isolated, or suspicious and non-infective, they attend the out-patient clinic.

At the out-patient clinic only suspicious cases and cases released on parole from the leper lazaret are treated. They are kept under constant supervision and repeated bacteriological tests are made to ensure that they are non-infective and not a source of any danger to non-infected friends. They sleep in houses separate from the other natives and the children attend special schools.

A leper lazaret has been established to ensure that strict segregation of infective cases is maintained. In addition the advanced cases are again segregated from the milder. A strip of the coast line, consisting of coral reef and foreshore, has been isolated. It is about a mile in length and in width varies from 100 to 200 yards. It is well supplied with water and food-bearing trees.

Discipline is maintained by two chiefs, assisted in their duties by district constables. The hospital staff consists of Nauruans, themselves lepers, the senior orderly before admission having held a similar post at the Government hospital, and the greater part of the injection work is performed by him. As an example of native efficiency, in 1927, 67,000 injections were given and abscess formation occurred in only ten cases. The hospital block consists of a central treatment department with two adjacent wards.

Intercurrent disease amongst those segregated is not very troublesome, and the number of deaths is very small and mostly from some form of tubercle infection. Any children born in the station are removed at birth and tended by relatives outside and do not appear to be infected.

Every facility is made of the following important factors in combating the disease:—

(*a*) Encouragement of personal cleanliness. Every home being situated along the foreshore, each patient takes his daily bath. In addition showers have been erected at various points, and rain catchment sheds and cement cisterns have been installed to ensure a good supply of potable water. Any cuts, abrasions or ailments receive immediate attention at the hospital. Fly-proof latrines are provided, and mass hookworm therapy is provided each half year.

(*b*) Cultivation of community spirit. Beyond the duties of keeping his own home and surrounding land clean, and sunning his sleeping mat each day, each patient is free to do as he chooses. Facilities have been provided by means of a 16-foot "No man's land" for talks with friends outside during daylight, and each is individually impressed with the nature and dangers of the disease, and his duty to the community at large.

(*c*) Diet. Here we have a very important weapon, and the ordinary native diet is encouraged, with a supplementary Government ration of wholemeal bread, brown rice, biscuits, sugar, cod-liver oil, and tinned milk.

(*d*) Open-air life. The native habits and life are unrestricted, and every encouragement is given to stimulate interest and intelligence. Schools are maintained for the children both indoors and out. There are churches, and a weekly picture-show of educational and humorous films. Sports grounds are provided, and interdistrict competitions are a regular feature. Physical training classes, boy scouts and girl guides have been instituted. Once a month at the entrance the native brass band renders selections. Fishing both for sport and food occupies a good deal of the natives' time. The Administrator attends each week, hears any suggestions or complaints, reads a brief résumé of the week's cable news and items of interest, and distributes books, magazines and pictorials kindly forwarded, after use, by well-wishers in Australasia.

(*e*) Specific treatment In summary the main agents that have been employed are—

(1) Chaulmoogra oil:
emulsion orally (to 1924);
Heiser's formula intramuscularly (to 1924);
crude oil intravenous, m. 3—20, 3—5 weekly (1924—6);
ethyl esters:—
Ecco (Muir) intramuscularly (1924)
Moogrol (B. W. & Co.) intramuscularly 5—21 c.c. weekly (1924—6)
intravenously (1926);

(2) Sodium hydnocarpate intravenously (1925)

(3) Sodium morrhuate 3% sol., 10 c.c. weekly (1925—6)

(4) Oleum hydnocarpus wightiana
ethyl esters } subcutaneously under lesions and intramuscularly, 4 c.c.
crude oil } daily, 6 days a week (1926—8)

(5) Trichloracetic acid, 1 : 5 sol., lesions painted weekly

(6) Potassium iodide orally.

At times intensive treatment has been given by combining several of the previous methods of introduction but all injection methods have been discontinued in favour of oleum hydnocarpus wightiana as the most reliable and effective weapon, and 4 c.c. can be given daily for months on end.

In conclusion it may be said that the natives have little fear of the disease, as the manifestations are mild and not of the repulsive type. They lead a natural, happy and normal life in segregation, under ideal conditions, with perfect hygiene, good dietary, and with every care and diversion. Their confidence is inspired by the number of friends they see released on parole each year and attend regularly for treatment. In fact their life in isolation is just as happy as, or happier than, it would be outside.

As a sign of good omen the Administration now looks upon the lowered incidence and the numbers released on parole, and is particularly pleased at the diminution in the number of children affected each year, as they appear to be the most susceptible class.

Section of Otology.

[*May* 2, 1930.]

Inclusion Cyst of Pharynx.—K. W. MACKENZIE, M.B.

Miss R., aged 36, had her tonsils removed by dissection in 1927. A few months later she began to complain of discomfort in the right side of the throat. This lasted about a week and was accompanied by a little swelling in the right tonsillar fossa, which would increase in size, discharge a little pus and then subside. The discomfort would then disappear. This process recurred about half a dozen times before she was admitted to hospital. Between the attacks the position of the swelling was marked by a little thickening, while the swelling itself was about the size of a bean.

August, 1929.—At operation, the swelling was easily stripped from the tonsillar bed and the pathologist reported it to be an "inclusion cyst" lying exposed on the raw surface of the pharynx; running slightly downwards and forwards was a nerve which appeared to be the glosso-pharyngeal. The fossa healed very quickly and the nerve was soon covered by granulation tissue.

Three or four weeks after the operation, the patient complained of a tingling sensation in the lobe of the right ear, particularly when exposed to cold air, and she had a feeling that the lobe was larger than normal. The tingling sensation disappeared after a few weeks, but the sensation of increased size was still present in February, 1930.

Discussion.—Dr. ALBERT A. GRAY said that this was an interesting exhibit because of the relationship between Arnold's nerve and the facial nerve.[1] Several years ago, at a meeting of this Section, he had read a paper dealing with this relationship, which varied in different individuals more than one would expect. Arnold's nerve was supposed to pass behind the facial and to send a twig into it, or to receive one from it. Sometimes the nerve did not pass through the bone at all, but went along the base of the skull. In other cases it sent a twig to the chorda tympani. The anatomy of that part of the temporal bone was not properly understood, because of the great variations which had just been mentioned. This little plexus in man was represented by a very large plexus in the sheep, and was outside the bone altogether. With the formation of the mastoid it had become included in the bone. The variation in the pain and subjective symptoms in cancer and other conditions in the throat were largely due to the variations in that small plexus between Arnold's nerve and the facial nerve.

Mr. HERBERT TILLEY, in supplementing Dr. Gray's observations, said that fibres from the small nerve (ramus facialis cutaneus) which supplied the back of the ear, and the deep meatal region, passed by way of the superficial petrosal to the geniculate ganglion and thence to the spheno-palatine ganglion, and explained the otalgia which a patient might complain of in chronic sphenoidal sinus inflammation. In 1892, at the Ipswich meeting of the British

[1] *Proceedings*, 1921, xv (Sect. Otol. 15).

Medical Association, he (the speaker) had brought forward a case of sphenoidal sinus suppuration in which the only symptom complained of was earache. There were no obvious symptoms of the sinus suppuration during the attacks, and the ear on the same side was intensely flushed in comparison with the other ear. There was an interesting reference to this subject by Larsell and Fenton, in their publication "The Embryology and Neurohistology of Spheno-palatine Ganglion Connections: a Contribution to the Study of Otalgia." (American Otological Society, Washington, D.C., May, 1928).

Dr. K. W. MACKENZIE (in reply) said he had looked up Gray's Anatomy, and found that the nerve supply to the lobule of the ear was from the great auricular, and that this nerve was connected with the posterior auricular. This in turn communicated with Arnold's nerve, which received a filament from the petrous ganglion of the vagus, and thus completed a chain between the pharynx and the lobe of the ear.

The Indications for Performing Wilde's Incision.

By T. B. LAYTON, D.S.O., M.S.

Sir WILLIAM WILDE[1], in describing acute myringitis, writes as follows:—

"Should the mastoid process, or the parts covering it, become engaged, and that the methods already recommended fail to give relief, or that even an indistinct sense of fluctuation can be discovered, we should not long hesitate to make a free incision in the periosteum there, at least an inch in length."

As is shown by the patients seen to-day, the operation that we now perform is not quite the same, as the incision need not be an inch long. The principle, however, is the same, in that the periosteum is freely incised.

I am unable to find any textbook to-day which speaks favourably of this operation, but it has always been performed in the hospitals of the Metropolitan Asylums Board for retro-auricular swellings occurring in the course of acute infectious diseases, and as it has shown itself undoubtedly to be of use I thought it worth while to bring it before you at the time when these hospitals have just passed under the care of the County Council. I feel sure that it is a procedure which can be employed in small children for conditions affecting the ear, other than those complicating the infectious diseases, and I believe that it should be used in a proportion of our cases both in hospital and private practice.

In all cases of infectious disease in which a retro-auricular swelling arises it is best to make a Wilde's incision. The two symptoms that call for immediate major operation under these circumstances are rigidity of the neck and the presence of rigors, and both of these are very rare. In my report to the Board in 1922, I said that a further operation on the bone should be done (1) if the temperature did not subside within forty-eight hours, (2) if the wound did not heal within a fortnight, and (3) if the ear was not dry within two months. In the years that have passed since that report was written I see no reason to change this as a general guide to treatment. We have, however, learned by experience that it is advisable to wait longer than forty-eight hours in many of the cases that come under the first heading, and that we can recognize which patients need a further operation earlier than at the time stated above in the cases that come under the second and third.

(1) In all severe attacks of diphtheria, scarlet fever, or measles, further operation should not be performed, if it can possibly be helped during the acute stage. In every case of diphtheria of moderate severity there is an affection of the heart-muscle which contra-indicates the administration of a general anæsthetic unless the otologist feels confident that the mastoid disease will kill the patient if he does not perform the operation. I have not yet had such a case in diphtheria.

[1] "A Treatise on Diseases of the Ear," 1853, p. 237.

Although scarlet fever is not recognized as affecting the heart-muscle in the way that diphtheria does, I am quite sure, as the result of my experience, that a bad case of scarlet fever is a dangerous anæsthetic risk and in all severe cases I refrain, if possible, from operating until the patients are well on their way to convalescence.

In measles the problem is not one of the heart so much as of the lungs. In every case of measles these are to a certain extent involved. Even when there is no pneumonia or even a severe bronchitis, there tends to be some trouble under the anæsthetic, leading to excess of bleeding of a troublesome nature, which prevents one seeing well and doing the bone work neatly. At the same time this must have a bad effect on the already damaged lungs. In severe cases of measles an anæsthetic is a definite risk and I feel confident that the patient in my second case would never have survived had a major operation under general anæsthesia been performed. This case is also interesting as an example of the fact that one can never be sure which case of retro-auricular swelling will recover with a simple incision. We were all confident that a mastoid operation would, finally, be necessary for this child, and we were merely waiting for her to get well enough to allow of our performing it, by which time the ears were dry. Operation should also be deferred in cases of scarlet fever even if the temperature does not subside, if there are enlarged glands in the neck. The temperature may be due to the glands rather than to the mastoid and so long as the glands are enlarged there is a chance of the ear clearing up without further operation. The same applies, though to a less extent, where albuminuria is a complication of the disease.

(2) When the wound does not heal one is able to recognize at the end of about a week that healing is not going to occur. One sees it healing at either end and a sinus developing as the intermediate part remains unhealed. When this happens, the further operation may be performed as soon as the patient's general condition admits. There is, however, no need to hurry, as in these cases the disease is usually limited to the surface of the mastoid without any extension to the deeper parts, although this will occur if the operation is deferred too long—say, for a period of from four to six months.

(3) I have now had ten years' experience in the Metropolitan Asylums Board Service and during this period I have never once opened a mastoid in which the wound from the Wilde's incision has healed, yet the ear discharge has continued, without finding some disease in the bone. Several cases similar to which I have shown to-day have led me to the conclusion that these are the cases in which there is a potentially serious condition—rather than those in which the wound does not heal. They should, therefore, all be operated on before they leave the fever hospital, and it is possible that they account for a certain number of the dangerous cases that used to be admitted to the general hospitals, following scarlet fever, with severe intracranial complications. Thus, if the patient shown to-day had left the hospital without an operation, at the next cold the extradural abscess would have become reinfected with an immediate invasion either of the sinus or of the meningeal cavity. Therefore, we do not now wait the full two months before deciding to perform this operation; we perform it as soon as we are confident that the ear is not healing up. If, however, the child has been severely ill, there is no reason to operate early and occasionally a case will get well in which operation had been decided upon. Thus, Marian H., aged 4½, was admitted to the Eastern Hospital with scarlet fever on October 29, 1929. On November 7 spontaneous left otorrhœa developed with a swelling over the mastoid, for which a Wilde's incision was performed on December 27. On February 13, 1930, we decided to operate on the mastoid at my next visit and permission for the operation was obtained, but on March 4, the day previous to that arranged for it, measles developed, and the operation was, therefore, deferred. We decided to perform it when the child had recovered from the measles, but by March 25 the wound and the ear had both cleared up.

It is usually held by those of great experience on the Board that Wilde's incision should not be tried in cases in which the swelling is in front of the ear, and in general I agree with this as these cases usually prove to be examples of zygomatic mastoiditis. In infants, however, in whom the cells have not yet developed, a Wilde's incision should be made in these cases as well. Wilde's incision is less likely to be successful in adults than in children, and when it is performed in an adult the case should be closely watched. Actually this problem does not often arise, as retro-auricular swelling is not common in infectious diseases in adults.

Turning from infectious diseases to the cases that one sees in hospital and private practice, I believe that we might apply to them the practice of the Metropolitan Asylums Board, though I do not think we should get so high a proportion of successes. In infants under three, however, I should always advise that a Wilde's incision be performed before the set mastoid operation is done, and it is, further, a most valuable operation for those living in the country to perform as a temporary measure where an otologist cannot be got at once. Some ten years ago Dr. Pennell had a child in the Edenbridge Cottage Hospital with a retro-auricular swelling seriously ill, with a temperature of 104°. He performed a Wilde's incision and showed the case to the visiting surgeon who was paying a routine visit a couple of days later. By this time all acute symptoms had passed and the child was very much better. The surgeon thought there was then no immediate need for further operation, and suggested the child should be transferred to me at Guy's Hospital. We were then able to perform the mastoid operation under favourable circumstances without having to operate across a large thickness of œdematous superficial tissues, and I think that this is a good principle to follow in many of these cases.

Lastly, just occasionally a retro-auricular swelling may occur in the adult, which will get well by a simple incision without further operation. I learnt this from the case of a patient whom I saw some seven years ago; I was asked to see him on a Sunday morning. He gave a story of severe pain having occurred in his ear some six weeks previously. After twenty-four hours the pain had improved, but deafness had set in. He subsequently had a recurrence of the pain, after which a swelling appeared behind his ear. When I saw him there was redness and some swelling of the drumhead, and a fluctuating swelling behind the ear. I advised operation, but he refused to have it done for a month. I told him I would not be responsible if he delayed longer than a week. He retorted by saying that he was willing to bear the responsibility himself. Towards the end of the month I wrote suggesting he should come to see me again with a view to making the arrangements for his operation. He replied that this would not be necessary, as shortly after I had seen him the swelling had burst, that it was discharging nicely, and he did not think it would be necessary to come to me again. I wrote and asked him to come as a matter of interest to me, and when he arrived there was a small scar above and behind the ear, which was healed, the drumhead was normal and the hearing had returned. I kept an eye on him for over a year, and there was no recurrence of the symptoms. I have been able to recognize the same condition once since, when we opened the swelling under local anæsthesia in the Out-patient Department, with complete recovery.

In conclusion, I do not believe that any of these cases that recover have ever been cases of disease of the mastoid, but in the great majority of cases it is impossible to tell in which the retro-aural swelling is secondary to disease of the bone, and in which the inflammatory products have come from the middle ear along the external auditory meatus and in that way reached the same spot. Therefore I think that this simple operative procedure should be performed in every case unless the symptoms are such that they command the major operation, and the more the otologist performs the minor operation the less often will he find these symptoms present.

ILLUSTRATIVE CASES.

Five Cases in which Wilde's Incision was successfully performed for Retro-aural Swelling.—T. B. LAYTON, D.S.O., M.S.

1.—J. D., male, aged 3 years. Moderate case of scarlet fever, with bilateral cervical adenitis, profuse rhinorrhœa and bilateral otorrhœa. One month after the onset of otorrhœa, œdema over right mastoid, with fever (101°); Wilde's incision; recovery; on discharge, drumhead and wound both healed.

II.—H. R. G., female, aged 7 years. Severe attack of measles in October. Double otorrhœa early in November. Swelling behind left ear December 6; Wilde's incision, no pus. Redness over right mastoid, January 20; Wilde's incision. February, both wounds healed and drumheads healed. Note: This child was extremely ill with bronchial pneumonia, and almost certainly would not have survived any operation at the time the first Wilde's incision was performed.

III.—(*shown by Mr. Layton and Dr. Laurent*).

B. B., female, aged 9 months. Scarlet fever in November, 1929. December, right-sided cervical adenitis, incised. January 15, 1930, spontaneous left otorrhœa. February 15, rise of temperature, red tender swelling over left mastoid. Wilde's incision; discharged from hospital with healed wound and dry ear.

IV.—(*shown by Mr. Layton and Dr. Laurent*).

W. C. B., male, aged 5 years, admitted January 1, 1930, on account of diphtheria, developed scarlet fever, while in hospital, on February 4. March 1, spontaneous double otorrhœa. March 8, slight œdema over right mastoid. No pyrexia. March 9, rise of temperature and œdematous swelling over right mastoid. Wilde's incision. Discharged April 1, incision and both drumheads healed.

V.—(*shown by Mr. Layton and Dr. Blake*).

J. M., female, aged 6 years, admitted on account of scarlet fever to Western Hospital in November and transferred to Northern Hospital, December 12, 1929. Intermittent pyrexia, January 10-15, with right ear discharge, which developed and persisted, gradually lessening in amount until February 15, when drumhead was intact, with a small scar on the lower posterior quadrant. February 16, complained of pain in right ear, accompanied by slight discharge, and at 10 a.m. on the same day swelling, redness and œdema over right mastoid. Wilde's incision done immediately, no pus found. Thick pus noted on February 21; the discharge lasted till March 17, and the otorrhœa until March 18.

A Case showing the danger of not performing a Mastoid Operation if Ear Discharge persists after a Wilde's Incision.—T. B. LAYTON, D.S.O., M.S.

Female, aged 5 years, admitted to Grove Hospital with a mild attack of scarlet fever on January 8. Retro-aural swelling and right otorrhœa on admission. Wilde's incision, pus evacuated. Temperature remained normal throughout. Child put on weight and looked very well. Wound healed, but otorrhœa persisted. Mastoid operation, February 15: Extradural abscess, with extensive granulations over the dura mater. Cultivations from cortical and subcortical bone, and from the pus all remained sterile.

Discussion.—The PRESIDENT said that Mr. Layton appeared to have made out a good case for performing Wilde's incision on children in fever hospitals.

Dr. DAN MCKENZIE said that Mr. Layton always did much good by compelling a reconsideration of first principles. What did Mr. Layton think that Wilde's incision did? Was it not just what the old doctors called counter-irritation? Would not a fly-blister bring about the same result? Wilde's incision could give some relief if there was a subperiosteal abscess, but if it was feared there was pus in the cells and the patient could not stand a long

operation, why not make a breach into the mastoid cortex? One then secured more drainage. If one made an incision through the soft parts and stopped there, one did not relieve the interior of the bone, except gradually, but if one took the gouge and eased the outer cortex of the mastoid and opened a cell, that was sufficient to relieve the pressure inside, at all events until the patient could stand the complete Schwartze operation.

Mr. E. A. PETERS said that twenty years ago, when he was at Paddington Green Children's Hospital, he had carried out Wilde's incision on six children. There was an epidemic of mastoiditis and the series of six could not be admitted. There was much retro-auricular swelling, with a raised temperature.

What had Mr. Layton found on making the incision? In two of his (the speaker's) own cases there had merely been serum, and in four there had been pus. Two of these latter had settled down, and it was only necessary to open the antrum eventually in two of the series. He thought the success of the incision was due—as had been pointed out by the late Mr. Arthur Cheatle—to the fact that in children the outer wall of the antrum was composed of thin spongy bone.

The drainage and relief of tension afforded by incising the periosteum resulted in resolution of the mastoiditis in four out of the six cases. The pus came through the bone in the antral region of the squamo-petrosal suture.

Mr. LOWNDES YATES said that soldiers during the War used to suffer from a swelling behind the ear, and an incision was made, and they were passed down to the Base. Many results were seen afterwards, and they were what one would expect if Mr. Layton's advice were followed out and if they were not operated upon later. The cortex of the bone became greatly thickened; the outer table was too strong for the pus to penetrate, and so it went through the inner table and formed an extradural abscess. Some of the cases gave a history of having refused a second operation, because they had already had an operation on the ear, and their ear was not hurting them. He had noticed that when the outer table became thickened as a result of the healing process, the pain was largely lost. Mastoid pain more probably occurred when the unsclerosed outer table was inflamed during the course of mastoiditis. His recollection was that in these cases pain was relieved by the incision, and that often it did not recur, although the inflammation in the mastoid was still active.

Mr. L. GRAHAM BROWN said he had never carried out Wilde's incision. He agreed that its indications were limited to those cases in which the patient could not stand a general anæsthetic. In other cases no time was lost in, at least, opening the bone. To his mind, the greatest difficulty was the healing after infectious diseases, especially after measles in these cases. Further, in acute mastoiditis in infants he had known a secondary infection to supervene, so that the stitches had come out of the wound and thus caused the ear to hang down. That misfortune could be obviated by using a limited incision. Instead of making the incision from the top of the pinna to the tip of the mastoid, he made it only over the mastoid process and down to the periosteum, laying the bone bare with a rougine, and opening up with a chisel. He then inserted the tube in the usual way for drainage. Thus one avoided much infection of the soft tissues, and especially of those over the temporal muscle.

Mr. C. DE W. GIBB said he was surprised that Mr. Layton had not mentioned the possibility of recurrence, even after healing, when Wilde's incision had been performed and the discharge had ceased. He (the speaker) had had such cases. When he had taken up his present appointment, he had an instance of a Wilde's incision performed by a general surgeon in a six-months-old infant. The discharge had persisted, and so he had removed some adenoid growths. Under treatment the discharge had ceased, and the infant was taken away with the drum normal; the incision had healed. Six weeks ago, however, the child was brought back to hospital having a typical mastoid infection. He (the speaker) had then performed a Schwartze operation, and the condition had cleared up. This case illustrated one of the disadvantages of Wilde's incision.

Mr. J. C. BLAKE said that in the last case shown by Mr. Layton, Wilde's incision had been performed immediately the swelling had been noticed, i.e., within two hours. When the child was discharged there was a completely healed drumhead, and the mastoid wound also had healed. So that in that instance the Wilde's incision had been a success, though one did not know that mastoid trouble might not follow later on. The temperature had come down after the Wilde's incision, and the child had made a normal recovery.

Mr. R. J. CANN said that during the four years that he was Mr. Layton's assistant at Guy's Hospital Mr. Layton had stimulated him to perform Wilde's incision in a general hospital, but he did not remember a case in which that procedure had been a success, i.e., one in which he did not have to do a further operation.

During the last year, at the Evelina Hospital, he had twice performed Wilde's incision on infants under a year old, in whom the general condition was not equal to an operation, indeed they could not have stood the anæsthetic. In the first cases in a marasmic baby, the incision was made without an anæsthetic; in the other case there had been pneumonia, and in that case he had expected to be obliged to perform a subsequent operation. But in three weeks' time healing had taken place in both these cases: there was no discharge, and the drums were intact.

Mr. LAYTON (in reply) said he was pleasantly surprised to find these views less unfavourably received than he had expected. He was, however, grieved to see that the Member whose criticism at a former meeting had caused him to prepare this paper had left the room before he read it, as had that other Member whose antagonism had been most vociferous in the private discussions on the cases in the Barnes Hall.

As a rule he did not advocate Wilde's incision for adults. Mr. Cann had mentioned cases which had not been successful. In infectious diseases such cases must be watched carefully, and in adults the chance of the Wilde incision being successful was so small that it was better to do the other operation straight away unless there was some reason against it. One feared complications in the adult when waiting, while there need be no such fear in children. The two cases he had recounted were absolutely isolated. With the first patient he had argued for two hours to try to persuade him to consent to the operation, as he had feared for the patient's life if he did not perform one.

Leaving out the question of adults, what did Wilde's incision do? Either there was disease of bone or there was not. If there was such disease, then Wilde's incision would not cure it. But it did relieve the tension, it mitigated the bad symptoms, and it was a good course of action until the patient was better. It was a principle in the surgery of acute disease to deal with the acute condition, and to clear up the underlying disease at a later stage. To perform the Wilde's incision was to carry out that principle.

In a case in which a Wilde's incision effected the patient's recovery he did not think there had ever been any inflammation of the bone, but that the products of inflammation had passed from the middle ear along the external auditory meatus to the point where the bony and cartilaginous meatus met and there turned backwards to the mastoid region. It was known that the middle ear could be infiltrated with an anæsthetic solution from the subperiosteal space of the meatus, therefore it was clear that the fluid exuded in an inflammation could pass the other way. That was how the serum or pus reached the meatus and why a swelling of the deep meatus was not necessarily evidence of inflammation of the mastoid, as was so often taught.

Now at every immobile joint the periosteum split and became continuous through the joint with that on the other side, therefore the potential space beneath this periosteum became continuous with that on the other side of the joint and not with the further space on the same side. Thus the potential space beneath the cutaneo-periosteum of the deep meatus was not continuous with the space beneath the cutaneo-perichondrium of the superficial meatus but with that beneath the periosteum over the side of the skull by the mastoid bone, and that was how the products of inflammation which had reached the deep meatus from the middle ear were guided to the same area as that in which the swelling appeared, with an inflammation of the mastoid bone. Twelve months ago he had had a case in which the incision had been carried out through the meatus. A large swelling of the deep meatus was present and he had thought it was a case in which a retro-aural swelling would arise. His house-surgeon (Mr. T. G. Scott) had incised the swelling and the child had recovered.

Once one had grasped the idea that a case of otitis media complicated by a retro-aural swelling might recover after a Wilde's incision, most of the questions he had been asked were answered, and most of the criticisms fell to the ground.

Thus, Dr. Dan McKenzie asked what the incision would do. The answer was that it would either effect a cure or put the patient in a condition of safety and prevent an extension in other directions. It had been suggested that a fomentation without incision would do as well. Undoubtedly in certain cases, but not where pus had formed, nor would it relieve the tension in cases in which the bone was involved, and one could not say which these were until the incision had been made.

Again, Dr. Dan McKenzie had asked why one did not take the gouge and ease the cortex. He (the speaker) used to do this, but had given up the practice. If the case was one that could be cured by an incision, this easing of the cortex was unnecessary, as there was no disease of bone. If there was disease of bone, the incision of the periosteum would relieve the tension and make the patient safe for the time being, and if it did not, then the complete mastoid operation should be done, and it was doubtful whether easing the cortex was sufficient. There was a further argument against it. The Wilde's incision could be made while the patient was in bed. With a small infant the crying was often no more than one heard when a nurse was washing a baby. For an older child he preferred a short anæsthetic, but there were many cases in diphtheria and measles and also in scarlet fever in which it was much better not to give the patient any anæsthetic at all.

There was another criticism that had been put to him. Surgeons had operated on cases of retro-aural swelling and found an extradural abscess with granulations on the sinus and had agreed that by performing the larger operation they had prevented a septicæmia. This was a wrong deduction : the relief of tension afforded by a Wilde's incision would have done the same. In such a case the major operation was better carried out at a later date than through an œdematous area of skin and superficial tissues.

So much for replies to destructive criticism. There were three points of constructive criticism on which he would like to comment. Mr. Lowndes Yates' experience of performing a Wilde's incision in a Field Ambulance or Casualty Clearing Station, and sending the patient on to a General Hospital, was quite in accordance with his (the speaker's) indication for the general practitioner working at some distance from an operating otologist. In reply to Mr. Gibb, he had not had recurrences after Wilde's incision. Of course it was possible for a patient who had had an incision done to have mastoid trouble later just as a recurrence under the old scar might happen after a mastoid operation. He had had a number of such cases, but thought they were fresh attacks and did not argue that at the original illness a larger operation should have been done. He suggested that in Mr. Gibb's cases the ear discharge had continued and the mastoid had not been opened. This was a course of action with which he had never agreed. Lastly, with reference to Mr. Graham Brown's comment on falling down of the pinna, it was characteristic of scarlet fever that the healing power of wounds was markedly diminished. Stitches were apt to cut through, so that the pinna fell down and made a valve of the meatus. To avoid this he had for several years refrained, whenever possible, from making the upper part of the usual curved mastoid incision, and had used only a straight incision over the lower area.

[The paper by Professor Maurice Soudille (Nantes) on "The Surgical Treatment of Otosclerosis" will be published in the next issue of the *Proceedings*.]

JOINT DISCUSSION No. 8.

Section of Dermatology and Section of Medicine.

[*March* 20, 1930.]

DISCUSSION ON THE THERAPEUTIC VALUE OF GOLD COMPOUNDS (SANOCRYSIN, KRYSOLGAN, ETC.).

Dr. S. E. Dore (Chairman): The therapeutic value of gold compounds, sanocrysin, krysolgan, etc., is a subject which presents some difficulties, and also some points of common interest to the two Sections.

The first difficulty which confronts us is that we are to some extent at cross purposes in this discussion. I take it that, as regards the use of the gold compounds from a distinctly medical point of view, only tuberculous conditions are treated, and that in such cases the ætiology is not in question. In dermatology, on the other hand, lupus erythematosus, which is the outstanding disease treated by the gold compounds, is of uncertain ætiology. It is considered by some to be definitely tuberculous, by others to be in some cases tuberculous, in others due to a streptococcal toxin produced by a septic focus in a distant part of the body. Again, some deny the importance of tuberculosis altogether in its ætiology. There is also considerable difference of opinion as to the value of the gold salts in the treatment of lupus erythematosus. I have heard an American dermatologist of standing state that the gold compounds are as specific in the treatment of lupus erythematosus as are the arsenobenzol preparations in that of syphilis, and a recent writer[1] compares their value with that of mercury in the treatment of syphilis. Another dermatologist[2] states in a recent publication that the gold compounds are extremely toxic, and cannot be used as routine measures.

An interesting point, which is perhaps of academical importance, is the bearing of the successful results obtained in lupus erythematosus on the ætiology of that disease. If it can be shown that these preparations are lethal to the tubercle bacillus, are we justified in assuming a tuberculous causation, or are the results obtained due to their action in causing local fibrosis or other pathological change? Among other points, which I think might be discussed, are the respective value of the various preparations of the gold salts—the dosage and spacing of the doses, their toxic effects, and the types of case suitable for treatment in this manner. It would be also interesting to hear what results have been obtained in glandular and other forms of "surgical" tuberculosis.

Professor S. Lyle Cummins: All those who have worked at the treatment of tuberculosis with the heavy metals have observed that these preparations, while well tolerated by healthy persons within the limits imposed by their pharmacological

[1] H. Haldin-Davis, "The Modern Treatment of Lupus Erythematosus." *British Medical Journal*, March 8, 1930, p. 432.

[2] R. M. B. MacKenna, "Treatment of Lupus Erythematosus with Bismuth." *Lancet*, Jan. 25, 1930, p. 178.

characters, are by no means so well tolerated by patients suffering from tuberculosis. In these latter, the administration of doses found to be quite innocuous to persons in health, is followed by reactions very similar to those produced by tuberculin.

Reactions of this kind were noted by von Linden as following the use of her copper compounds and by Feldt as caused by his gold preparations, aurocantan and krysolgan, before sanocrysin had been discovered; and Møllgaard, when he described so well the contrasting effects of his new preparation in healthy and tuberculous animals respectively, was recapitulating almost exactly the findings of his predecessors in the field of heavy metal therapy in tuberculosis.

My own experience in the treatment of tuberculosis by gold compounds is limited to sanocrysin, and so I propose to confine my remarks to this preparation which, apart from the fact that it appears to possess certain advantages over the others in its clinical applicability, may be taken as representative of the whole group.

Sanocrysin, at first unduly vaunted as a "cure" or rashly damned as a poison, is now gaining general acceptance as a useful adjunct in the treatment of tuberculosis. It was vaunted as a "cure" because the apparently successful cases were not observed over a sufficient length of time. It was damned as a poison because it *is* a poison, just as surely as tuberculin is a poison, when used in large doses in severe cases of advanced tuberculosis, and it was only when thus improperly used that it gave rise to lethal effects. It is interesting to look back over the last five years and recall the various phases in medical opinion as to sanocrysin. They represent a departure from what is usual in the history of new treatments in tuberculosis. Most of the new remedies advocated in this disease, being without harmful—or any other—effects, have an early success—depending, very largely, upon "suggestion"—which gives place rapidly to oblivion. In the case of sanocrysin, its actively harmful effects, so potent in the hands of inexperienced persons, led to an early phase of unpopularity. In 1906, only about eighteen months after the first publication of clinical results with sanocrysin, a very great authority on tuberculosis treatment asserted, at the International Congress on Tuberculosis at Washington, that "sanocrysin therapy was already dead." Thanks to the persistent optimism of the Danish physicians and, in this country, to the quiet and critical work of several tuberculosis specialists, notably Clark, of Belfast, and Heaf, of the Warwickshire Sanatorium, sanocrysin treatment has been not only stripped of its terrors but gradually placed in its correct position, neither as a "cure" nor as a "poison" but as a valuable adjuvant in the management of tuberculosis.

During the last few years, instead of dropping back into the limbo of "new remedies" it has tended to be more and more used in our hospitals and sanatoria, in spite of its high cost and in spite of the disagreeable reactions which, no matter how skilled the physician, appear to be inseparable from its use, at least in occasional cases. And the whole trend of published reports has undergone a change. Instead of papers demonstrating its failure in a few patients, we have articles comparing the progress of treated and untreated groups of human cases, usually to the decided advantage of the former. Faber, Permin, Wurtzen and Sporslav, Jaquerod, Gruenfeld, and others, while far from claiming sanocrysin as a cure, agree that it is of value. The results gained by Frimodt-Moller in the treatment of Indian patients show clearly that, even in relatively severe cases in a class of patients recognized as liable to the more progressive forms of tuberculosis, it may be used with excellent effect.

My own experience of sanocrysin has been definitely favourable. Some cases have improved only to relapse, and a very few have failed to improve at all, but, for the most part, there has been a decided change for the better, and several patients, of a type not usually amenable to treatment, are still alive and well after three, four and five years since treatment. But it is not my intention to describe a

series of cases, or even to embark on the risks and difficulties of the treatment, which, indeed, have been exaggerated. What I wish to discuss is the mode of action of the gold compounds, taking sanocrysin as the type of which I happen to have most experience, because it seems to me that without some definite idea on this point, our attempts to employ these preparations must remain empirical.

Møllgaard, in his earlier communications, like von Linden in relation to her copper salts, explained the action of sanocrysin as being due to its bactericidal qualities, complicated by the liberation of toxic substances from the killed bacilli. This conception dominated his work from the start and led on naturally to the use of anti-tuberculous serum along with the gold salt. To quote his own words, "the introduction of any chemical compound which kills tubercle bacilli, into a seriously infected tuberculous organism must produce a very severe intoxication unless something can be done to neutralize the liberated toxins." The worst of this hypothesis is that it is opposed to the results of experiments. Sanocrysin, although it has the power of inhibiting the growth of tubercle bacilli in culture, provided it be present in relatively high concentration, is powerless to kill tubercle *in vitro*. Møllgaard, in a later paper, read before the Section of Comparative Medicine in this Society in April, 1927, fully recognized this fact and expressed himself as follows: "The difference between the effects *in vitro* and *in vivo*, the latent period, and the importance of the condition of the infected organism, may be due to the fact that the chemotherapeutical compound is transformed into active bodies by chemical reactions in the organism and that the bodies thus formed are the true parasitotrophic substances which cause the death of the parasites." This hypothesis is hard to reconcile with the fact that the simultaneous inoculation of sanocrysin and tubercle bacilli leads on to general tuberculosis; the sanocrysin obviously being just as inert in bactericidal power in the living blood as in the test tube. The experiments of Fry, in which sanocrysin was found to be powerless to prevent the growth of tubercle bacilli in blood-clots, according to Wright's technique, and which I have repeated with identical results, show that no bactericidal substances are developed, at least from the cells included in the whole blood, within the period of a week or ten days, the duration of the experiment. It seems clear that we must drop the bactericidal hypothesis. And yet the sharp difference between the effects of sanocrysin in the tuberculous and the healthy, as well as the close similarity between the reactions to sanocrysin and those to tuberculin, point clearly to a specific action of sanocrysin in the tuberculous organism.

Feldt, Walbum and others hold that the heavy metals possess a catalytic action on the infected tissues. While this catalytic hypothesis requires for its substantiation a far more detailed knowledge of what happens in the organism than we at present possess, it appears to me that we are safe in postulating, like Feldt and others, an indirect action of sanocrysin in tuberculosis. Against this Møllgaard says: "When we find that the therapeutical effect of a certain substance increases with the dosage up to the limit of its 'dosis tolerata,' we may conclude that quantitative relations must enter into consideration in chemical reactions in which the parasite is included, and this may further suggest that the parasite is submitted to direct chemical influence, derived not from the unaltered substance, but from the products derived from its interaction with the tissues of the infected organism." But in my own experience with sanocrysin I find that its therapeutical effect is often just as great with small doses as with large. Here, of course, there is a risk of a misunderstanding through a faulty use of words. If Møllgaard includes "shock" amongst "therapeutical effects," then his statement is incontrovertible. But, if by "therapeutical effects" are understood beneficial effects, then it is safe to say that in the average tuberculous patient small doses have a decided "therapeutical effect," and large doses, at least at the beginning of treatment, may do harm.

The increased toleration of sanocrysin which undoubtedly comes into play during the course of successful treatment may be taken as an index of the return of the patient towards that normal and healthy state in which the "dosis tolerata" may be given without disagreeable results.

It may be claimed, of course, that what we ought to aim at is a large dosage, and that shock, if it occurs, may easily be neutralized by anti-tuberculous serum. Secker still holds to this theory. Like most of those who have worked with sanocrysin, I have learnt that it is advisable to *avoid* shock and do without the added risks of serum therapy. I find in the fact that "therapeutical" results are usually to be obtained by small as well as large doses, a striking proof that the action of sanocrysin is, for the most part, indirect. Everything points to its action being an indirect one through the stimulation of the tuberculous foci rather than a direct bactericidal action upon the tubercle bacilli.

In tuberculous animals killed shortly after a dose of sanocrysin, it is found that the tuberculous foci are markedly hyperæmic. Eichelburger and McClusky have shown that the excretion of sanocrysin in the urine of tuberculous animals is about ten per cent. less than in healthy animals, given the same doses, indicating that there is a tendency to retention of the gold salt in the tuberculous tissues. Gallinal, using a microchemical technique, has been able to show that the gold is located around the foci and is particularly visible in the tuberculous organs. If this view of the focal action of sanocrysin be accepted, then it will be apparent that the effects of a given dose may be expected to vary in different patients according to the types of focal lesion from which they are suffering. This is exactly what happens in practice. It is not correct to say that the larger the dose, within the limits of the "dosis tolerata," the greater the therapeutical effect. It is more nearly correct to say that, for each individual patient, there is an optimum scale of dosage which, as in vaccine therapy, must be arrived at partly by a correct appreciation of the patient's condition and partly by the method of trial and error.

If, as appears possible from the observations of Gallinal, the gold preparation is to some extent deposited in the neighbourhood of tuberculous foci, it is conceivable that sufficient may accumulate there to produce an inhibition effect upon the multiplication of tubercle bacilli in the tuberculous tissue—just as sanocrysin is known to do *in vitro* when in sufficient strength—and some such action may, perhaps, serve to help on the effects of focal stimulation, but the fact that small doses are often capable of producing markedly beneficial effects suggests that this inhibition action, if it occurs at all, is of secondary importance.

In my own experiments in animals, as well as in the treatment of cases, one salient point has been borne in upon me: that sanocrysin can only act beneficially in organisms already possessed of a sufficient degree of resistance to tuberculosis to enable them, on the one hand, to tolerate the results of sharp focal reactions and, on the other, to exploit these reactions as a means towards the healing up of the focus after stimulation.

My attempts to treat tuberculosis in guinea-pigs by means of sanocrysin have invariably failed, these animals appearing incapable of acquiring sufficient resistance to tolerate severe focal reactions.

In experimentally infected rabbits, on the other hand, especially when the human type of tubercle bacillus, against which the rabbit possesses considerable resistance, has been used to induce a chronic focal type of tuberculosis, the effects produced by me with sanocrysin have been very marked, the treated animals, when killed at an interval of about three months after the gold salt inoculations, being found practically free from disease, whereas the untreated "controls" still showed very extensive tuberculosis.

It is the same with human beings. Those showing, in their response to infection, some evidence of resisting power, are found to be able to tolerate the

auto-inoculations resulting from sanocrysin and to derive marked benefit as the results of the focal stimulation produced by this preparation. On the other hand, the non-resistant type of case seldom reacts favourably to this form of treatment.

If Møllgaard, instead of saying that "the therapeutical effect . . . increases with the dosage up to the limit of the 'dosis tolerata'" had merely said that "the effect" so increased, he would have been, I think, more correct, in the sense that the larger the dose, the greater the disturbance of the tuberculous foci. But the effects of very marked disturbance of acute focal lesions are usually detrimental, not "therapeutical," and, as Møllgaard has so clearly shown, include "shock" and other very undesirable sequelæ. Therapy should aim at the avoidance of such effects. The object should be to produce the maximum of benefit to the patient, with the minimum of risk. To gain this object it is essential to arrange the scale of dosage to suit the individual case.

Within the limits of the "dosis tolerata," the dose, that is, which can be given to a healthy person without risk of metallic poisoning, the selection of the appropriate dosage for the individual patient depends rather upon the laws which regulate vaccine therapy than on those which are applicable to drugs without a chemotherapeutic action on the tuberculous tissues. The art of sanocrysin therapy lies in the just appreciation of the patient's condition and the selection of the doses and the intervals between doses accordingly.

With these points in mind, it is obviously desirable to turn for a moment to the consideration of tuberculous foci, as seen in pathological studies on man and animals, with regard to their structure, their content, and, more especially, their variations in different types of case. The immature tuberculous foci in non-resistant animals or persons are of the nature of small caseo-necrotic tuberculous abscesses in which the central parts consist of dead or dying cells, cellular and nuclear débris, and, as a rule, numerous tubercle bacilli in rapid multiplication, while at the periphery is, at most, a thin layer of recently formed granulation tissue, the only barrier between the healthy structures and the progressive tuberculous lesion. The avascular centre of such a focus is soaked with the soluble products of bacillary multiplication and disintegration, as well as of cellular destruction, and there is only a narrow cellular layer between these toxic products and the capillaries at the periphery. The ease with which auto-inoculations can take place from such foci is evidenced by the rise of temperature following any attempt at exertion or any factor leading to focal hyperæmia in early acute cases. Such foci, if numerous, seldom go with any marked power of resistance, and sanocrysin, if given in any but the smallest doses, may cause "shock" or otherwise do a great deal of harm. But I have found in cases of this type that very small doses, as low as one-tenth or even one-twentieth of a gram, may be followed by marked improvement, as shown by a return of the temperature to normal and a slowing down of the blood sedimentation rate.

In the secondary tubercles of more resistant cases, the "acute abscess" phase, if it ever occurred, has been reduced to a more or less limited caseous centre surrounded by well-formed granulation tissue in which proliferative changes are taking place. There is a tendency to the production of giant cells and fibroblasts in the epithelioid layer and, at the periphery, a considerable amount of new fibrous tissue and numerous lymphocytes, the whole representing an organized structure in which the growth of bacilli and the poisoning of tissue cells are roughly balanced by the processes of spontaneous cure and an increased power to neutralize such toxic products as may gain access to the blood-stream. It is easy to see that sanocrysin, by causing hyperæmia of such foci, may give access to a flood of antibodies and phagocytes and that the resultant washing out of toxic substances, while causing the pyrexial and other reactions usually seen to follow moderate doses of sanocrysin in such cases, will, in itself, constitute an important factor in the cure of the lesion.

When the tuberculous lesions are, for the most part, of this type, sanocrysin may be expected to be well tolerated and to give good results.

There is still a third type of focus in which, although it may often contain living tubercle bacilli, the processes of spontaneous cure have so far advanced as to shut out the germs and their products from the blood-stream on the one hand and to shut off the blood-stream from the bacilli on the other. When these processes of spontaneous cure have so far advanced as to form a barrier impermeable to substances dissolved in the circulating blood, sanocrysin cannot conceivably be of any use. It is the existence of foci of this type which explains the relapses of patients who had at first appeared to be quite restored to health by sanocrysin treatment. The slow processes in the central parts of these foci are capable of gradual progression until the fibrous and cellular barriers are eroded at some point and an autogenous re-infection takes place. In cases of a type likely to harbour foci of this sort, it is never wise to assume that a single course of sanocrysin, even though followed by apparent cure, is sufficient. Further courses, at intervals, are necessary, with the idea of a sort of " fractional sterilization " of the lesions which may, conceivably, lead to ultimate cure in favourable cases.

But how, it may be asked, is it possible to decide whether any given case has this, that, or the other type of tuberculous lesions? It is easy to classify foci in dead tissues, but how are they to be evaluated in the living body? I think that Marcus Paterson, with his application of Wright's theory of auto-inoculation to the " grading " of work and exercise, and the late Dr. Inman, with his classification of cases according to their physical response to exercise and rest, have pointed out at least one of the methods by which the state of the tuberculous foci in living patients may be arrived at. When there is pyrexia even at rest, the unbalanced type of early focal disease may be assumed to exist and sanocrysin, if given at all, must be given with the greatest caution. Where pyrexia is absent at rest but excited by exercise, soon subsiding again on a return to rest, it may be assumed, on the one hand, that hyperæmia can stimulate auto-inoculations and, on the other, that these are soon neutralized. This type of case has proved, in my experience, to be particularly favourable for sanocrysin treatment, especially in young subjects. Where the patient is free from any rise of temperature even on hard exercise, it is useless to expect dramatic results from sanocrysin. Some results there may be; but I have found that such cases are better left to physiological treatment in sanatoria.

There are, however, many ways, in addition to a study of the temperature, by which cases may be assessed.

In the search for characters, according to which cases may be evaluated and compared, it will be advisable to turn more and more towards ponderable and measurable changes, rather than to those criteria which depend for their significance upon the individual skill and experience of the physician. Amongst our surest guides are the temperature, the weight, and the amount, character and bacillary content of the sputum. The great advances now being made in chest radiography, the recent progress in the study of blood changes in tuberculosis and a better appreciation of the significance of tuberculin sensitivity afford profitable lines along which future work may be directed in the assessment and comparison of cases before, during and after treatment.

Dr. A. C. Roxburgh and **Dr. H. Corsi**: The forty-two cases referred to, except seven, have been treated by us in the Skin Out-patient Department of St. Bartholomew's Hospital. Two were private cases and five were treated at St. John's Hospital.

The diseases, which we have treated with gold compounds, have been five in number, viz.:—

Lupus erythematosus (thirty-five cases), Bazin's erythema, induratum scrofu-

losorum (four cases), papulo-necrotic tuberculides (one case), erythrocyanosis frigida (one case), pityriasis lichenoides et varioliformis acuta (one case).

We shall therefore deal chiefly with lupus erythematosus. The gold compounds used have been two: krysolgan and sanocrysin.

Krysolgan was first prepared by Feldt in 1917. It is sodium-amino-auro-mercapto-benzol-carbonate: formula, $C_6H_3NH_2$ (SAu) COOH. Molecular weight 387. Gold content approximately 50%. In aqueous solution it does not give the reactions of the gold ion. Solutions of 1/100,000 inhibit the growth of the tubercle bacillus. The doses we have used have been ·0015 gr. to 1·5 gr. (= ·1 grm., or approximately ·05 grm. of gold).

Sanocrysin was prepared by Møllgaard in 1925. It is a gold and sodium thiosulphate; formula $(AuS_2O_3)Na$, $Na_2S_2O_3$. It dissociates in solution into S_2O_3 + 3Na + AuS_2O_3, the latter being the therapeutic agent. The molecular weight of sanocrysin is 492. Gold content approximately 40%. It is said to kill the tubercle bacillus in dilutions of 1 in 100,000, and to inhibit growth in 1 in 1,000,000. The doses we have used have been ·05 grm. (i.e., 500 times the minimum dose of krysolgan) to ·25 grm. (i.e., two and a half times the maximum dose of krysolgan, and containing approximately twice the amount of gold). Sanocryin has been given elsewhere in very much larger doses than this, but all our cases have been treated as out-patients, and, as we were unwilling to risk unpleasant reactions, the dose of ·25 grm. has not been exceeded.

Each dose has been dissolved in one or two c.c. of distilled water and given intravenously. The intervals have usually been one week between the smaller doses and two weeks after the maximum dose has been reached. Longer intervals have been allowed in some cases, e.g., after reactions. The total dosage of krysolgan has been as much as 29 gr. in forty-three injections, spread over twenty months, and of sanocrysin 4·25 grm. in twenty-two injections, spread over thirteen months. The average number of injections of krysolgan was eleven and of sanocrysin, ten.

The patients treated with these drugs had no local treatment (except calamine lotion) given at the same time, although the majority of the cases of lupus erythematosus had had local caustics such as carbon dioxide snow, carbolic acid, etc., applied previously; consequently any improvement noticed can be regarded with fair probability as being due to the gold salt injected.

The majority of the cases of lupus erythematosus treated have been of the chronic fixed type, the longest duration before treatment with gold salts began, being twenty years, and the shortest one month, the average being seven years. Only five cases reported a duration of twelve months or less before beginning gold treatment.

The sex distribution was nineteen females and sixteen males. The age limits were 18 to 61 years for the females (average 36), and 11 to 58 for the males (average 42).

On the average, as one would expect, the patients who did best were those who had had the disease the shortest time. Thus in six cases of lupus erythematosus, temporarily "cured," the disease had lasted respectively one month, six months, one year, three years, four years, and ten years: average three years. In four cases vastly improved, it had lasted four months, two years, five years, and sixteen years: average 5·8 years. In three cases showing no change, it had lasted three years, seven years, and seventeen years: average nine years. In four cases, made worse, it had lasted four years, ten years, twelve years, and sixteen years: average 10·5 years.

As the gold salts were primarily introduced for the treatment of tuberculosis, we thought it desirable to ascertain, as well as we could, which of our patients showed evidence, or gave signs of the presence of tuberculous foci, and which did not, for we may remind those of you who are not dermatologists, that lupus erythematosus

is generally considered to be produced by the action of light on a skin sensitized by the toxins of either the tubercle bacillus or the streptococcus.

Of our forty-two patients four had strong clinical evidence of tuberculosis apart from cutaneous tests. (Table I.) Four gave positive results to both Mantoux and Pirquet tests, but otherwise had no clinical evidence of tuberculosis apart from the disease (lupus erythematosus or Bazin's disease). (Table II.)

Five patients gave positive results to the Mantoux test. (Table III.) Three gave negative results to the Mantoux test. (Table IV.) The remaining twenty-six patients either were not tested, or did not return at the proper time to have the result read. That is to say, out of sixteen patients thirteen gave evidence of the presence of tuberculosis and three gave evidence of its absence.

The four cases of Bazin's disease may, we suppose, be accepted as being tuberculous, and of these one was cured, one improved, and two uninfluenced. Of the twelve cases of lupus erythematosus tested, nine may be taken as being tuberculous and three not.

The results in the tuberculous cases were three vastly improved, three improved, one uninfluenced, and two worse. Of the supposedly non-tuberculous cases one was cured, one vastly improved, and one uninfluenced. The numbers are much too small to provide reliable data, but, so far as they go, they suggest that the gold salts may cause improvement in lupus erythematosus independently of the presence of tubercle bacilli, for the proportions of those improved to those not improved are exactly the same, viz., 2 to 1, in each group.

The results of the Mantoux test in these cases are interesting in that they seem to support strongly the Continental view that lupus erythematosus is, in the majority of cases, due to the presence of tuberculosis rather than the view held in this country that it is at least as often due to streptococcal infection.

Comparison of the Two Drugs.—In all, thirty-five cases of lupus erythematosus were treated with krysolgan or sanocrysin, and the comparative results with the two are given in Table V. The figures in this table represent the results of *courses of treatment*, not cases, as several cases had first krysolgan and then sanocrysin.

All the eight cases "cured" relapsed in a few months except two, one treated with krysolgan, and one with sanocrysin. These have only ceased treatment fairly recently, and may yet relapse. Only two cases became worse with sanocrysin against five with krysolgan. The results with sanocrysin were seen more rapidly than with krysolgan, possibly owing to the larger dose. Table VI shows the results in those cases which were treated first with krysolgan and secondly with sanocrysin. Summarizing these results it appears that out of six cases sanocrysin effected an improvement over and above that produced by krysolgan in three cases, in two it produced no further change (except, in one case, great loss of weight), and one case, made worse by krysolgan, was made still worse by sanocrysin.

The results in Bazin's erythema induratun scrofulosorum were as follows. (Table VII.)

That is to say, one recent case was cured by only four injections of krysolgan, one older and more severe was only improved after forty-three injections, one was unaltered by six injections and one by twelve, the latter however, improving, after six injections of sanocrysin.

The single case of papulo-necrotic tuberculides, without Bazin's erythema, was improved after eight injections, totalling 1·749 gr. of krysolgan. The single case of erythrocyanosis frigida crurum was improved after seven injections (1·394 gr.) of krysolgan, and the single case of pityriasis lichenoides et varioliformis acuta was uninfluenced after four infections (0·099 gr.) of krysolgan.

The ill effects recorded have been as follows. (Table VIII.)

In three cases, one with krysolgan and two with sanocrysin, the exacerbations were severe and extensive, although temporary, and followed by no permanent

TABLE I.

CASES WITH STRONG EVIDENCE OF TUBERCULOSIS APART FROM CUTANEOUS TESTS.

No.	Initials	Sex	Age	Signs and symptoms	Duration	Drug	No. of injections	Total dose	Duration of treatment	Result
9	H.B.	F.	57	Old phthisis. L.E. face, neck, scalp	5 years	Krysolgan	7	0·1905 gr.	2 months	Vastly improved
28	N.C.	F.	19	Recent but inactive phthisis, L.E.	4 years	Krysolgan	7	0·257 gr.	4 months	Worse
15	M.K.	F.	20	Thickened pleura, right base	2 years	Krysolgan	12	4·074 gr.	2½ months	No change
				Bazin. Pap-nec-tuberculides hands	—	Sanocrysin	6	0·75 grm.	4months	Improved
31	C.B.	F.	18	T.B. glands neck. Bazin	3 months	Krysolgan	6	0·625 gr.	—	No change

Summary.—Two lupus erythematosus: one vastly improved, one worse. Two Bazin's disease: one improved, one uninfluenced.

TABLE II.

CASES WITH BOTH MANTOUX AND PIRQUET TUBERCULIN TESTS POSITIVE.

No.	Initials	Sex	Age	Disease	Distribution	Duration	Drug	No. of injections	Total dose	Duration of treatment	Result
11	E.B.	F.	46	L.E.	Ears, nose, cheeks, hands	16 years	Krysolgan	6	0·399 gr.	2 months	Worse
							Sanocrysin	7	0·75 grm.	3 months	Worse
17	H.G.	M.	33	L.E.	Face	10 years	Krysolgan	17	12·033 gr.	9 months	Improved
16	G.P.	F.	19	Bazin	Legs	2 years	Krysolgan	43	29·337 gr.	20 months	Improved
35	V.W.	F.	26	Bazin	Legs	6 months	Krysolgan	4	0·0855 gr.	1 month	Cured

Summary.—Two lupus erythematosus: one improved, one worse. Two Bazin's disease: one cured, one improved.

TABLE III.

CASES OF LUPUS ERYTHEMATOSUS WITH MANTOUX TEST POSITIVE.

No	Initials	Sex	Age	Distribution	Duration	Drug	No. of injections	Total dose	Duration of treatment	Result
3	B. B.	F	34	Scalp, face, very severe, extensive	16 years	Krysolgan	31	15·834 gr.	13 months	Vastly improved
						Sanocrysin	12	2·65 grm.	5 months	No change except loss of weight
6	A. J.	M	41	Left ear	7 years	Krysolgan	7	0·484 gr.	2 months	No change
29	J. McC.	M	46	Ears, cheeks, neck	2 years	Sanocrysin	9	1·9 grm.	3 months	Vastly improved, relapsed 5 mths. later
40	J. D.	M	49	Nose	4 years	Sanocrysin	4	0·46 grm.	1 month	Improved
43	E. C.	F	39	Nose	15 years	Sanocrysin	7	0·6 grm.	2 months	Improved

Summary.—Five cases of lupus erythematosus. Two, vastly improved. Two, improved. One, no change.

TABLE IV.

CASES OF LUPUS ERYTHEMATOSUS WITH MANTOUX TEST NEGATIVE.

No.	Initials	Sex	Age	Distribution	Duration	Drug	No. of injections	Total dose	Duration of treatment	Result
8	A. W.	M	18	Face, neck, hands	4 months	Krysolgan	20	4·406 gr.	5 months	Vastly improved (later completely relapsed)
34	R. E	M	11	Cheeks	3 years	Krysolgan	6	0·1155 gr.	1½ months	No change
44	R. R	F	27	Nose	4 years	Krysolgan	8	3·999 gr.	2 months	Cured

Summary.—Three cases of lupus erythematosus. One cured. One, vastly improved. One, no change.

TABLE V.

LUPUS ERYTHEMATOSUS, 35 CASES. COMPARISON OF RESULTS OF KRYSOLGAN AND SANOCRYSIN.

A patient treated by both drugs appears in both columns.

Result	Drugs	
	Krysolgan	Sanocrysin
"Cure"	3	5
Vast improvement	2	4
Definite improvement	8	4
Slight improvement	1	1
No change	4	4
Worse	5	2
Total courses of treatment, 43	23	20

TABLE VI.

CASES TREATED FIRST WITH KRYSOLGAN AND THEN WITH SANOCRYSIN.

No.	Initials	Sex	Age	Disease	Duration	Evidence of T.B.	No. of injections Krysolgan	Total dose of Krysolgan	Result of Krysolgan	No. of injections Sanocrysin	Total dose of Sanocrysin	Result of Sanocrysin
11	E. B.	F.	46	L.E. ...	16 years	Mantoux + Pirquet +	6	0·399 gr.	Worse ...	7	0 75 grm.	Worse
3	B. B.	F.	34	L.E. ...	16 years	Mantoux +	31	15·834 gr.	Vastly improved	12	2·65 grm.	No further improvement; lost weight
14	A. H.	M.	20	L.E. ...	5 years	Not tested	12	0·514 gr.	Definite improvement	9	1·8 grm.	No further improvement
d.	A. V.	F.	36	L.E. ...	7 years	Not tested	7	1·999 gr.	Definite improvement	12	2·65 grm.	Great further improvement
13	E W.	F.	48	L.E. ...	6 months	Not tested	17	7·899 gr.	Definite improvement	22	4·25 grm.	Cured
15	M. K.	F.	20	Bazin's papnec-tuberculides	2 years	Thickened pleura Pirquet +	12	4 074 gr.	No change ...	6	0·75 grm.	Improved

TABLE VII.

BAZIN'S ERYTHEMA INDURATUM SCROFULOSORUM

No.	Initials	Sex	Age	Signs and symptoms	Duration	Drug	No. of injections	Total dose	Duration of treatment	Result
15	M. K.	F	20	Bazin's. Thickened pleura. Pap-nec-tuberculides. Pir-quet +	2 years	Krysolgan Sanocrysin	12 6	4·074 gr. 0·75 grm.	2½ months 4 months	No change Improved
31	C. B.	F	18	Bazin's. T.B. glands in neck. Pirquet +	3 months	Krysolgan	6	0·625 gr.	—	No change
16	G. P.	F	19	Bazin's. Pirquet + Mantoux + +	2 years	Krysolgan	43	29·337 gr.	20 months	Improved
35	V. W.	F	26	Bazin's. Pirquet +	6 months	Krysolgan	4	0·0855 gr.	1 month	Cured

TABLE VIII.

ILL EFFECTS OF GOLD INJECTIONS.

	Fever	Malaise	Joint pains	Headache	Vomiting	Œdema of eyelids	Pruritus of hands	Temporary exacerbation of L. E.	Stomatitis (severe)
Krysolgan, 349 injections	1	3	2	3	1	1	1	5	0
Sanocrysin, 194 injections	0	1	3	4	2	0	0	3	1

ill-effects. These exacerbations followed respectively the seventh injection of krysolgan, and the fourth and eighth of sanocrysin in different cases. It is possible that they represented a dermatitis due to gold. The latter patient after her eighth injection of sanocrysin developed an irritating erythemato-squamous rash on neck and vest area, and, when it subsided, a condition of atrophy and pigmentation on the sides of the neck, resembling the "poikilodermie" of Civatte, was noticed.

To sum up: thirty-five cases of lupus erythematosus were treated by injection of one or other or both gold salts. Eight were cured, but only temporarily. Six were vastly improved, nine were definitely improved, two were a little improved, five were uninfluenced, and five were worse. Local treatment was not applied during the courses of gold injections.

Toxic reactions followed about 6% of the injections. They were not important except in three cases, in which either a mild gold dermatitis or an extensive exacerbation of the disease was produced. Even in these, however, the trouble was temporary. In one case a severe stomatitis followed the ninth injection of sanocrysin.

Out of twelve cases of lupus erythematosus tested, nine gave evidence of the presence of tuberculosis and three gave evidence of its absence (negative Mantoux's test). There was no difference in the proportion of successes and failures with gold treatment between the tuberculous and the non-tuberculous cases, although the numbers were too small to allow of firm deductions.

It appears, therefore, that both these drugs are of considerable value in the treatment of lupus erythematosus, although neither of them can be regarded as a cure for the disease.

There seems to be little to choose between the two drugs. Sanocrysin is perhaps slightly more effective, but it is also slightly more toxic in the doses given, of which the maximum was two and a half times that of krysolgan. It also seems to produce its effect more quickly, no doubt for the same reason.

Dr. Geoffrey Marshall said that he had been using sanocrysin in the treatment of pulmonary tuberculosis since it was first introduced by Møllgaard and Secker, of Copenhagen. He had found that it was unwise to give this treatment in the early stages of tuberculous disease, so that his experience of its effects in human beings was parallel with the experimental results which Professor Lyle Cummins had obtained in animals. At first, sanocrysin was given in all kinds of cases of medical tuberculosis and the result in some cases was disastrous. One of the worst he had seen was that of a young woman with a tuberculous pleural effusion of only a few weeks' duration. Two large injections of sanocrysin were followed by a very severe pyrexial reaction, and she developed a pyopneumothorax from which she died shortly after. By delaying the administration of sanocrysin for at least a year from the commencement of tuberculous disease, these dangerous reactions could be avoided.

With regard to the type of disease for which the gold compounds were suited, Professor Cummins gave Inman's classification, but did not say how far he allowed it to influence him. Inman divided cases into those which were febrile when at rest, those which were not febrile unless they were exercising, and those which were afebrile even when exercised. He (the speaker) considered it would be a mistake to fear to give sanocrysin to patients who were febrile when at rest, as he had seen the greatest benefit in those cases. He had had a number of patients who had been febrile in spite of prolonged rest, even absolute rest in bed and under the most careful constitutional treatment for many months. Beginning with small doses, he had, after pyrexial attacks, reduced the temperature lower, until eventually the patient was afebrile, and, finally, up and about, remaining free from fever. Some of the patients still remained free from symptoms, though most of them had relapses.

He would not claim to have cured any patient of pulmonary tuberculosis by means of gold injections alone. He had had many cases of that disease in which the symptoms had ceased after a course of the treatment, but there were definite relapses, moreover, his experience was that they relapsed more quickly after gold injections than after constitutional treatment. Still, it was a great acquisition to have a means of getting patients fit for constitutional treatment or collapse therapy.

He had not been successful in the fractional sterilization method suggested by Professor Cummins, though he had given it a conscientious trial.

Dr. H. W. Barber said that his experience with gold compounds corresponded fairly closely with that of Dr. Roxburgh and Dr. Corsi. He had treated many cases of lupus erythematosus during the last few years. He began with krysolgan, then treated some patients with sanocrysin, and lately he had been using solganal, which, it was claimed, was less toxic and more efficacious than krysolgan. Of the three preparations named he had had the most dramatic and permanent results from sanocrysin, but that, as Dr. Corsi had said, might be because the dosage was larger. His custom was to use small and slowly progressive doses, but he thought it was worth while in a resistant case to give what could be termed a "knock-out" dose, provided there was no tuberculosis of the lung. He remembered the case of a boy who had very extensive and rather acute lupus erythematosus of the face, with evidence of old tuberculosis. He began treatment with 0·25 grm. sanocrysin, later rising to 0·5 and eventually 0·75 grm. As a result of the last injection the patient had high pyrexia for ten days, and he was very ill; but the lupus erythematosus cleared up completely and there had been no relapse. Another case, which had not improved under small doses of solganal, had cleared up when he gave full doses, though the patient had been upset for a time.

One case of the acute erythematous form of lupus erythematosus—which was he considered, as a rule, streptococcal in origin—was of interest. It was that of a young woman, in whom the eruption began acutely after follicular tonsillitis, and he had feared that the fatal form of the disease might develop. Small doses of sanocrysin were given, but they were followed by no rise of temperature or reaction. The Mantoux test was practically negative, but the patient gave a strong reaction to intradermal tests with streptococcal vaccine.

He had treated several cases of sarcoid with solganal or krysolgan, and, on the whole, he thought those in which there was the small nodular form of sarcoid did very well. Some cases had cleared up completely under the treatment, and, so far, there had been no relapse. One extensive case, with deep nodular lesions and deposits in the bones, shown to the Section in his (the speaker's) absence, had improved only slightly. Some of his tuberculide cases had done well, but he could not claim any of them as cures.

He had seen more toxic reactions after sanocrysin than after the other two preparations. After solganal he had seen a generalized lichenoid rash with severe stomatitis; it was a case of pulmonary tuberculosis brought to him for an opinion on the rash. He had heard of only one case of exfoliative dermatitis following injections of solganal at his clinic, but he did not see it. The case, which developed a lichenoid rash, was alcoholic, and that fact had an important bearing on the liability to toxic reactions, for, he thought, whether these patients developed toxic reactions in the skin depended largely on the extent to which the liver could deal with the drugs.

Summing up, he considered that the gold treatment of lupus erythematosus was satisfactory up to a point. Some cases had remained permanently well over a period of two or three years, but many others had relapsed.

Dr. J. E. M. Wigley said that his experience of the gold treatment in dermatological cases had been entirely limited to sanocrysin, and he had used it only

in lupus erythematosus. His experience had closely corresponded with that of Dr. Roxburgh, and presumably also with that of Dr. Barber. He had treated twenty-one cases in this way, and perhaps he had been rather fortunate in that only one of those cases was completely unaffected. None of his cases was made worse by the treatment. All the twenty cases were improved, though in some the improvement was only temporary.

There was one case, however, which seemed to him of sufficient interest to mention. It was that of a man aged 42, who had had lupus erythematosus of the "fixed" type, affecting the typical bat's wing area, the ears, and forearms, and was of two years' duration. There had been no definite history of illness, and he (the speaker) asked a physician to examine the man carefully. This was done, and the chest was also examined by X-rays. No evidence was found of tuberculous disease, but the patient had chronic bronchitis and emphysema. He had five injections of sanocrysin, receiving altogether 1·6 grm. Then an eczematous eruption suddenly developed on the forearms, legs and thighs, which itched intensely, and reminded one of the ordinary eczematous eruption following injections of novarsenobillon. The patient was taken into the hospital, and the whole condition cleared up in a fortnight. While he was there, the biochemist to Charing Cross Hospital, Dr. Paterson, mentioned that he had seen, in the reports of the French Biological Society,[1] a test for gold which was said to be simple and sensitive. It depended on the appearance of a characteristic violet coloration when the specimen containing gold was incinerated, heated with hydrochloric acid, evaporated to dryness, and ignited. The test was rendered more sensitive by extracting the ignited material with water, dissolving the residue in bromine or chlorine water, evaporating to dryness, and then re-igniting. The test was given by ·001 of a mgm. of gold. A small portion of the eczematous skin of this patient was removed and treated in this way, and there was a positive reaction showing that gold was present in the skin. A piece of skin was removed from another patient with lupus erythematosus, who had been given practically the same amount of sanocrysin, and the reaction was completely negative, and in a portion of skin removed from a cadaver, the reaction was negative. This test seemed to show that there was some gold present in the reacting skin. Coquin stated that the test showed gold to be present in the urine, sometimes within ten minutes of an injection, sometimes some weeks afterwards, and he had also found gold in the saliva.

Dr. H. MacCormac said that the principles of treatment of pulmonary tuberculosis by gold compounds were, he thought, not equally applicable to dermatological conditions for two reasons: first, only half the field of attack was available because the lesions were on the surface, and secondly, many believed that some forms of lupus erythematosus were not tuberculous. He was much interested to learn that Professor Cummins' experiments showed that it was necessary for the tubercle bacillus to become fixed in the tissues and to excite a local reaction before the gold treatment could be effective. A parallel was found in syphilis, in which it had been observed that the intravenous injection of the arsenical compounds before the appearance of the primary sore did not cure or abort the disease, but only delayed the development of the symptoms. These observations demonstrated that the therapeutic effects of these remedies were indirect, for, although the remedies were in a sense specific, they required for their effective action the co-operation of the tissues.

He had tried sanocrysin in about half a dozen cases of lupus vulgaris with only moderate success. All had improved, but in none had he observed anything approximating to the results seen in many cases of lupus erythematosus.

[1] *Compt. Rend. Soc. Biol.*, 1925, xciii,584-586.

Dr. W. N. Goldsmith said that he had no tabulated results to submit, but his experience with the use of gold compounds in lupus erythematosus had been very extensive, as, since 1925, he had treated every case of that condition with some gold compound. His general impression was that it was an extraordinary advance in the treatment of the disease. By far the greater number of the cases had greatly improved. Some had apparently been cured, and had remained well for about three years. The majority relapsed after varying intervals, but were again improved after further treatment.

He had not used sanocrysin, because of the numerous reports of toxic effects on the kidneys. He had used krysolgan, triphal, aurophos and lopion. He had also tried metallic colloidal gold, but this had had no effect whatever; several cases, showing no response to colloidal gold, suddenly improved when krysolgan or triphal were substituted. On the whole triphal gave the quickest results. This was now being withdrawn by the maker in favour of lopion, which was less toxic, at any rate to animals.

Generally he had used very small doses. With triphal he began with 0·001 grm., and seldom found it necessary to use a larger dose than about 0·025. The dosage was graded not by any fixed scheme, but according to the response to each injection. It was only increased (one and a half times) when there was an absence of any response for better or worse. Most of the cases did well, in spite of the absence of a local reaction.

If there was a temporary aggravation, he had usually diminished the dose, but his impression was that those patients did less well in any case. Those did best who never had a clinically obvious focal reaction. The dosage was puzzling. One man had had a fixed type of lupus erythematosus for over twenty years. On being treated with small doses of triphal he rapidly improved till scarcely a trace of the disease remained. He was then given the same dose as three weeks previously. There ensued a very severe flare, completely changing the clinical picture from the fixed to the diffuse erythematous type, so that the patient was worse than when he first came for treatment.

On the other hand, a patient who had been steadily improving on the usual small doses was one day, by mistake, given five times the dose intended, but the improvement merely continued rapidly without any unpleasant reaction.

The first case gives point to the conception of this treatment as an irritation or "Reiz-therapie," whereas the second is more consistent with the idea of catalytic action.

If there was any analogy between this method and the use of novarsenobenzol in syphilis, treatment ought probably to be continued for some time after the disappearance of the lesions, if relapses were to be prevented. At present we had no guide as to how long the treatment should continue.

Professor Cummins (in reply) said that he had been greatly interested in the remarks of the dermatologists who had spoken; those who were treating medical cases with gold had much to learn from them.

In answer to Dr. Marshall, some of the best cases the speaker had had were of the resting febrile type. What he had wished to insist on was that one must use great caution in regard to doses in cases of that kind. By limiting oneself to Secker's technique one could not treat that sort of case without running the risk of shock. He urged that one should think of the type of focus in these acute cases which were getting auto-inoculations spontaneously, even when at rest; the doses must, in such cases, be very small. Sometimes, however, sanocrysin, carefully used, had an extraordinary effect in lowering the temperature in cases of this kind.

Dr. Goldsmith's remark that he found these gold salts acted better when there was no obvious hyperæmia of the lesion, was interesting, but he (the speaker)

thought he would find there were, perhaps minor degrees of hyperæmia invisible to the naked eye. Probably a visible hyperæmic reaction in cases of lupus erythematosus might be too severe. Just as Dr. Goldsmith had found in some of his skin cases, so he (Professor Cummins), too, had noted in one or two pulmonary cases, that when one had continued to treat the case for a long time, without disagreeable reactions and with steady improvement, a "repeat dose" might give rise to a sudden exacerbation; a fact which might mean that the lesion had been made a little more "naked" as a result of successive hyperæmias, and so an unexpectedly severe reaction might result from a dose previously well tolerated. He submitted this merely as a suggestion.

Dr. Corsi (in reply) said that with regard to toxic symptoms, the physician and the dermatologist were in different positions. The cases of lupus erythematosus treated were of the ambulatory afebrile type, if any, of the tuberculous conditions. The toxic symptoms, therefore, were probably only of the kind which could occur in a normal person, and not due to a lighting-up of tuberculous foci.

He had been interested to hear of the 0·75 grm. "knock-out" dose of sanocrysin. At St. Bartholomew's Hospital they had not exceeded 0·25 grm., yet toxic symptoms had been frequent, even though ultimately not serious.

With regard to collosol gold: Møllgaard in his original paper stated that it was the ion AuS_2O_3, which had the power to diffuse through the avascular tissue surrounding the tuberculous follicle. Collosol gold would not have this power of penetration.